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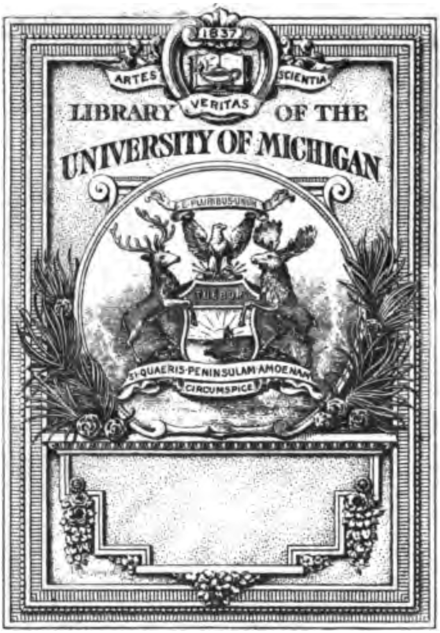
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THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 352. 1905. Vol. XXVIII.

INTRODUCTION.

The present 'Companion' closely resembles that of last year.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Lœwy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides, which gradually increase in number. The occultations of stars by the Moon, the phenomena of Jupiter's Satellites, and the diagrams and Ephemerides of the Satellites of the other planets are from the 'Nautical Almanac.' The Ephemeris of Jupiter V. is from the 'Connaissance de Temps,' 1905.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used, and the astronomical day is reckoned from noon to noon, except in the Ephemerides of Variable Stars, where Greenwich Civil Time is used, the day being reckoned from midnight to midnight.

Date.	Day of Year.	Fr. of Year.	Sun Sets.	Sun Rises.	☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
1905.			h m	h m	° ′	h m s	h m s	d h m
Jan. 1	1	·000	4 0	8 8	23 2 S	0 3 32	18 41 52	Jan.
8	8	·019	4 8	8 6	22 18	6 43	19 9 28	● 5 6 17
15	15	·038	4 17	8 2	21 12	9 28	19 37 4	☽ 13 8 11
22	22	·058	4 29	7 54	19 46	11 40	20 4 40	○ 20 19 14
29	29	·077	4 41	7 45	18 2	13 14	20 32 16	☾ 27 12 20
Feb. 5	36	·096	4 54	7 34	16 2	14 10	20 59 52	Feb. ● 3 23 6
12	43	·115	5 6	7 22	13 49	14 26	21 27 28	☽ 12 4 20
19	50	·134	5 19	7 9	11 24	14 3	21 55 3	○ 19 6 52
26	57	·153	5 32	6 54	8 51	13 8	22 22 39	☾ 25 22 4
Mar. 5	64	·173	5 44	6 40	6 12	11 45	22 50 15	March. ● 5 17 19
12	71	·192	5 57	6 24	3 28	10 1	23 17 51	☽ 13 21 0
19	78	·211	6 9	6 8	0 42 S	8 2	23 45 27	○ 20 16 56
26	85	·230	6 20	5 52	2 3 N	5 54	0 13 3	☾ 27 9 35
April 2	92	·249	6 32	5 36	4 47	3 46	0 40 39	April. ● 4 11 23
9	99	·268	6 43	5 21	7 26	1 45	1 8 14	☽ 12 9 41
16	106	·288	6 55	5 5	9 59	23 59 55	1 35 50	○ 19 1 38
23	113	·307	7 6	4 50	12 24	58 22	2 3 26	☾ 25 23 14
30	120	·326	7 18	4 36	14 39	57 11	2 31 2	
May 7	127	·345	7 29	4 24	16 42	56 27	2 58 38	May. ● 4 3 50
14	134	·364	7 40	4 12	18 32	56 11	3 26 14	☽ 11 18 46
21	141	·383	7 50	4 2	20 6	56 21	3 53 50	○ 18 9 36
28	148	·403	8 0	3 54	21 24	56 58	4 21 26	☾ 25 14 50
June 4	155	·422	8 8	3 48	22 23	57 59	4 49 2	June. ● 2 17 57
11	162	·441	8 13	3 45	23 4	59 16	5 16 37	☽ 10 1 5
18	169	·460	8 18	3 44	23 24	0 44	5 44 13	○ 16 17 52
25	176	·479	8 19	3 45	23 25	2 14	6 11 49	☾ 24 7 46
July 2	183	·498	8 18	3 49	23 5	3 40	6 39 25	July. ● 2 5 50
9	190	·518	8 15	3 55	22 25	4 53	7 7 1	☽ 9 5 46
16	197	·537	8 9	4 3	21 27	5 46	7 34 37	○ 16 3 32
23	204	·556	8 0	4 12	20 10	6 14	8 2 13	☾ 24 1 9
30	211	·575	7 51	4 21	18 38	6 14	8 29 49	● 31 16 3
Aug. 6	218	·594	7 39	4 33	16 50	5 46	8 57 25	August. ☽ 7 10 17
13	225	·613	7 27	4 43	14 48	4 47	9 25 0	○ 14 15 31
20	232	·633	7 12	4 54	12 36	3 22	9 52 36	☾ 22 18 10
27	239	·652	6 58	5 6	10 13	1 33	10 20 12	● 30 1 13
Sept. 3	246	·671	6 42	5 17	7 43	0 0 33	10 47 48	Sept. ☽ 5 16 9
10	253	·690	6 26	5 27	5 6	2 54	11 15 24	○ 13 6 10
17	260	·709	6 11	5 39	2 25 N	5 22	11 43 0	☾ 21 10 14
24	267	·728	5 55	5 50	0 18 S	7 49	12 10 36	● 28 10 0
Oct. 1	274	·747	5 39	6 1	3 2	10 10	12 38 12	Oct. ☽ 5 0 54
8	281	·767	5 22	6 13	5 44	12 17	13 5 47	○ 12 23 3
15	288	·786	5 7	6 25	8 22	14 4	13 33 23	☾ 21 0 51
22	295	·805	4 53	6 37	10 55	15 23	14 0 59	● 27 18 58
29	302	·824	4 39	6 49	13 20	16 10	14 28 35	
Nov. 5	309	·843	4 25	7 3	15 34	16 19	14 56 11	Nov. ☽ 3 13 39
12	316	·862	4 14	7 14	17 36	15 48	15 23 47	○ 11 17 11
19	323	·882	4 5	7 26	19 23	14 36	15 51 23	☾ 19 13 34
26	330	·901	3 57	7 37	20 53	12 44	16 18 59	● 26 4 47
Dec. 3	337	·920	3 52	7 48	22 3	10 14	16 46 34	Dec. ☽ 3 6 38
10	344	·939	3 49	7 56	22 53	7 17	17 14 10	○ 11 11 26
17	351	·958	3 50	8 2	23 21	3 58	17 41 46	☾ 19 0 9
24	358	·977	3 53	8 7	23 26	0 30	18 9 22	● 25 16 4
31	365	·997	3 58	8 8	23 8 S	57 2	18 36 58	

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	16 16	21 8	14 7 S	−39·3 E.
2	M	17 25	22 2	16 38	−51·4 E.
3	Tu	18 28	22 50	18 11	−63·6 E.
4	W	19 24	23 50	18 43	−75·8 E.
5	Th	sets	......	18 14	−88·0 E.
6	F	5 16	0 42	16 49	+79·8 M.
7	S	6 19	1 32	14 37	+67·6 M.
8	**S**	7 23	2 20	11 47	+55·4 M.
9	M	8 26	3 5	8 30	+43·3 M.
10	Tu	9 30	3 49	4 56	+31·1 M.
11	W	10 32	4 31	1 12 S	+18·9 M.
12	Th	11 35	5 13	2 35 N	+ 6·8 M.
13	F	12 38	5 56	6 17	− 5·4 M.
14	S	13 41	6 39	9 46	−17·5 M.
15	**S**	14 45	7 25	12 55	−29·7 M.
16	M	15 50	8 13	15 33	−41·9 M.
17	Tu	16 54	9 4	17 29	−54·0 M.
18	W	17 53	9 58	18 32	−66·1 M.
19	Th	18 48	10 54	18 32	−78·3 M.
20	F	rises	11 51	17 24	+89·6 E.
21	S	5 9	12 49	15 8	+77·5 E.
22	**S**	6 26	13 46	11 53	+65·4 E.
23	M	7 43	14 41	7 53	+53·2 E.
24	Tu	9 3	15 35	3 25 N	+41·1 E.
25	W	10 22	16 28	1 12 S	+29·0 E.
26	Th	11 38	17 20	5 40	+16·8 E.
27	F	12 53	18 12	9 46	+ 4·7 E.
28	S	14 7	19 4	13 15	− 7·5 E.
29	**S**	15 16	19 57	15 58	−19·7 E.
30	M	16 20	20 51	17 46	−31·9 E.
31	Tu	17 16	21 43	18 35 S	−44·0 E.

FEBRUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	18 6	22 35	18 25 S	−56·2 E.
2	Th	18 48	23 25	17 19	−68·4 E.
3	F	sets	......	15 24	−80·6 E.
4	S	5 11	0 14	12 48	+87·2 M.
5	**S**	6 14	1 0	9 41	+75·0 M.
6	M	7 17	1 44	6 12	+62·8 M.
7	Tu	8 19	2 27	2 30 S	+50·6 M.
8	W	9 24	3 10	1 16 N	+38·4 M.
9	Th	10 25	3 52	4 58	+26·3 M.
10	F	11 27	4 34	8 30	+14·1 M.
11	S	12 30	5 18	11 44	+ 1·9 M.
12	**S**	13 32	6 4	14 31	−10·3 M.
13	M	14 36	6 52	16 42	−22·4 M.
14	Tu	15 34	7 43	18 7	−34·6 M.
15	W	16 32	8 37	18 35	−46·7 M.
16	Th	17 24	9 33	17 59	−58·9 M.
17	F	18 9	10 30	16 15	−71·0 M.
18	S	18 48	11 28	13 26	−83·2 M.
19	**S**	rises	12 26	9 41	+84·7 E.
20	M	6 37	13 22	5 16	+72·6 E.
21	Tu	7 58	14 17	0 32 N	+60·5 E.
22	W	9 20	15 12	4 11 S	+48·3 E.
23	Th	10 38	16 6	8 33	+36·2 E.
24	F	11 55	17 0	12 20	+24·0 E.
25	S	13 6	17 54	15 19	+11·8 E.
26	**S**	14 13	18 47	17 22	− 0·3 E.
27	M	15 12	19 40	18 25	−12·5 E.
28	Tu	16 4	20 32	18 29 S	−24·7 E.

MARCH.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	16 49	21 22	17 38 S	−36·9 E.
2	Th	17 24	22 10	15 56	−49·1 E.
3	F	17 57	22 57	13 32	−61·3 E.
4	S	18 23	23 41	10 34	−73·5 E.
5	**S**	sets	......	7 11	−85·7 E.
6	M	6 10	0 25	3 33 S	+82·1 M.
7	Tu	7 14	1 7	0 12 N	+69·9 M.
8	W	8 16	1 49	3 57	+57·7 M.
9	Th	9 18	2 32	7 32	+45·5 M.
10	F	10 20	3 15	10 51	+33·3 M.
11	S	11 22	3 59	13 45	+21·1 M.
12	**S**	12 24	4 46	16 6	+ 8·9 M.
13	M	13 24	5 34	17 44	− 3·3 M.
14	Tu	14 19	6 26	18 33	−15·5 M.
15	W	15 12	7 19	18 22	−27·6 M.
16	Th	15 59	8 14	17 8	−39·8 M.
17	F	16 40	9 10	14 50	−52·0 M.
18	S	17 15	10 6	11 32	−64·1 M.
19	**S**	17 48	11 3	7 24	−76·3 M.
20	M	rises	11 59	2 44 N	−88·4 M.
21	Tu	6 50	12 55	2 8 S	+79·4 E.
22	W	8 12	13 52	6 50	+67·3 E.
23	Th	9 33	14 48	11 2	+55·1 E.
24	F	10 49	15 44	14 27	+43·0 E.
25	S	12 2	16 40	16 54	+30·8 E.
26	**S**	13 6	17 34	18 17	+18·6 E.
27	M	14 1	18 28	18 37	+ 6·5 E.
28	Tu	14 48	19 19	17 58	− 5·8 E.
29	W	15 27	20 8	16 27	−17·9 E.
30	Th	16 0	20 55	14 12	−30·1 E.
31	F	16 29	21 40	11 22 S	−42·4 E.

APRIL.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	16 53	22 23	8 5 S	−54·6 E.
2	**S**	17 18	23 6	4 30	−66·8 E.
3	M	17 38	23 48	0 45 S	−79·0 E.
4	Tu	sets	......	3 2 N	+88·8 M.
5	W	7 10	0 30	6 42	+76·5 M.
6	Th	8 12	1 13	10 8	+64·3 M.
7	F	9 15	1 57	13 10	+52·1 M.
8	S	10 17	2 43	15 41	+39·9 M.
9	**S**	11 17	3 31	17 31	+27·7 M.
10	M	12 13	4 20	18 33	+15·5 M.
11	Tu	13 7	5 12	18 40	+ 3·3 M.
12	W	13 53	6 4	17 48	− 8·9 M.
13	Th	14 35	6 58	15 55	−21·1 M.
14	F	15 13	7 52	13 3	−33·3 M.
15	S	15 44	8 47	9 19	−45·5 M.
16	**S**	16 15	9 42	4 56	−57·7 M.
17	M	16 45	10 37	0 8 N	−69·8 M.
18	Tu	17 14	11 33	4 42 S	−82·0 M.
19	W	rises	12 30	9 15	+85·8 E.
20	Th	8 23	13 27	13 10	+73·7 E.
21	F	9 40	14 25	16 9	+61·5 E.
22	S	10 50	15 22	18 3	+49·3 E.
23	**S**	11 52	16 18	18 48	+37·2 E.
24	M	12 44	17 12	18 27	+25·0 E.
25	Tu	13 27	18 3	17 9	+12·8 E.
26	W	14 4	18 52	15 3	+ 0·6 E.
27	Th	14 33	19 38	12 18	−11·6 E.
28	F	14 58	20 22	9 5	−23·9 E.
29	S	15 24	21 5	5 32	−36·1 E.
30	**S**	15 44	21 47	1 48 S	−48·3 E.

MAY.

DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	16 7	22 29	2 1 N	−60·5 E.
2	Tu	16 28	22 11	5 46	−72·8 E.
3	W	16 53	23 55	9 19	−85·0 E.
4	Th	sets	......	12 31	+82·8 M.
5	F	8 10	0 41	15 14	+70·5 M.
6	S	9 12	1 28	17 18	+58·3 M.
7	**S**	10 10	2 17	18 34	+46·1 M.
8	M	11 5	3 8	18 56	+33·8 M.
9	Tu	11 54	4 0	18 19	+21·6 M.
10	W	12 36	4 53	16 43	+ 9·4 M.
11	Th	13 12	5 46	14 10	− 2·8 M.
12	F	13 46	6 39	10 46	−15·0 M.
13	S	14 17	7 32	6 41	−27·2 M.
14	**S**	14 44	8 25	2 7 N	−39·4 M.
15	M	15 11	9 18	2 39 S	−51·6 M.
16	Tu	15 42	10 13	7 19	−63·8 M.
17	W	16 16	11 9	11 33	−76·0 M.
18	Th	rises	12 7	15 1	−88·2 M.
19	F	8 29	13 5	17 29	+79·7 E.
20	S	9 36	14 3	18 47	+67·5 E.
21	**S**	10 34	15 0	18 55	+55·3 E.
22	M	11 23	15 54	17 57	+43·1 E.
23	Tu	12 2	16 45	16 5	+30·9 E.
24	W	12 36	17 33	13 28	+18·7 E.
25	Th	13 2	18 18	10 19	+ 6·5 E.
26	F	13 27	19 2	6 49	− 5·7 E.
27	S	13 51	19 44	3 4 S	−18·0 E.
28	**S**	14 11	20 26	0 46 N	−30·2 E.
29	M	14 34	21 9	4 35	−42·4 E.
30	Tu	14 58	21 52	8 15	−54·7 E.
31	W	15 23	22 37	11 37 N	−66·9 E.

JUNE.

DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	15 53	23 24	14 32 N	−79·2 E.
2	F	sets	......	16 51	+88·6 M.
3	S	8 5	0 13	18 25	+76·4 M.
4	**S**	9 2	1 4	19 4	+64·1 M.
5	M	9 54	1 57	18 44	+51·9 M.
6	Tu	10 37	2 50	17 22	+39·6 M.
7	W	11 17	3 43	15 3	+27·4 M.
8	Th	11 50	4 36	11 51	+15·1 M.
9	F	12 21	5 28	7 58	+ 2·9 M.
10	S	12 49	6 20	3 36 N	− 9·3 M
11	**S**	13 15	7 11	1 2 S	−21·5 M.
12	M	13 44	8 4	5 39	−33·7 M.
13	Tu	14 13	8 57	9 59	−45·9 M.
14	W	14 49	9 52	13 44	−58·1 M.
15	Th	15 27	10 49	16 37	−70·3 M.
16	F	16 15	11 47	18 27	−82·5 M.
17	S	rises	12 45	19 6	+85·3 E.
18	**S**	9 14	13 40	18 37	+73·1 E.
19	M	9 59	14 34	17 5	+60·9 E.
20	Tu	10 35	15 24	14 43	+48·7 E.
21	W	11 6	16 12	11 42	+36·5 E.
22	Th	11 32	16 57	8 15	+24·3 E.
23	F	11 56	17 40	4 31	+12·1 E.
24	S	12 17	18 22	0 40 S	− 0·1 E.
25	**S**	12 39	19 5	3 11 N	−12·3 E.
26	M	13 1	19 47	6 56	−24·6 E.
27	Tu	13 27	20 31	10 25	−36·8 E.
28	W	13 54	21 17	13 32	−49·0 E.
29	Th	14 26	22 6	16 7	−61·3 E.
30	F	15 5	22 57	17 59 N	−73·5 E.

JULY.

DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	15 51	23 49	18 59 N	−85·8 E.
2	**S**	sets	......	18 59	+82·0 M.
3	M	8 36	0 43	17 57	+69·7 M.
4	Tu	9 18	1 38	15 52	+57·5 M.
5	W	9 54	2 32	12 51	+45·2 M.
6	Th	10 26	3 25	9 6	+33·0 M.
7	F	10 54	4 17	4 48	+20·7 M.
8	S	11 22	5 9	0 14 N	+ 8·5 M.
9	**S**	11 48	6 0	4 22 S	− 3·7 M.
10	M	12 17	6 52	8 44	−15·9 M.
11	Tu	12 47	7 45	12 36	−28·2 M.
12	W	13 25	8 40	15 43	−40·4 M.
13	Th	14 8	9 36	17 54	−52·6 M.
14	F	14 58	10 32	18 59	−64·7 M.
15	S	15 55	11 28	18 56	−76·9 M.
16	**S**	rises	12 23	17 49	−89·1 M.
17	M	8 32	13 14	15 46	+78·7 E
18	Tu	9 6	14 4	12 59	+66·5 E.
19	W	9 33	14 50	9 40	+54·3 E.
20	Th	9 59	15 35	5 59	+42·1 E.
21	F	10 22	16 18	2 8 S	+29·8 E.
22	S	10 43	17 0	1 45 N	+17·7 E.
23	**S**	11 5	17 42	5 32	+ 5·5 E.
24	M	11 30	18 25	9 8	− 6·8 E.
25	Tu	11 54	19 10	12 22	−19·0 E.
26	W	12 25	19 57	15 9	−31·2 E.
27	Th	13 2	20 46	17 18	−43·5 E.
28	F	13 43	21 38	18 39	−55·7 E.
29	S	14 34	22 32	19 4	−68·0 E.
30	**S**	15 34	23 27	18 26	−80·2 E.
31	M	16 42	......	16 42 N	+87·5 M.

AUGUST.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	7 53	0 22	13 58 N	+75·3 M.
2	W	8 27	1 17	10 21	+63·1 M.
3	Th	8 57	2 11	6 6	+50·8 M.
4	F	9 26	3 4	1 29 N	+38·6 M.
5	S	9 54	3 57	3 12 S	+26·3 M.
6	**S**	10 21	4 49	7 40	+14·1 M.
7	M	10 52	5 42	11 40	+ 1·9 M.
8	Tu	11 25	6 36	14 57	−10·3 M.
9	W	12 6	7 30	17 21	−22·5 M.
10	Th	12 52	8 26	18 44	−34·7 M.
11	F	13 46	9 21	19 1	−46·9 M.
12	S	14 45	10 14	18 14	−59·1 M.
13	**S**	15 49	11 7	16 31	−71·3 M.
14	M	rises	11 56	13 59	−83·5 M.
15	Tu	7 35	12 44	10 51	+84·3 E.
16	W	8 1	13 29	7 17	+72·1 E.
17	Th	8 26	14 13	3 28 S	+59·9 E.
18	F	8 48	14 56	0 26 N	+47·7 E.
19	S	9 10	15 38	4 16	+35·6 E.
20	**S**	9 33	16 20	7 56	+23·4 E.
21	M	9 58	17 4	11 17	+11·2 E.
22	Tu	10 25	17 49	14 12	− 1·1 E.
23	W	10 56	18 37	16 33	−13·3 E.
24	Th	11 36	19 27	18 12	−25·5 E.
25	F	12 21	20 19	18 59	−37·7 E.
26	S	13 18	21 13	18 47	−50·0 E.
27	**S**	14 20	22 8	17 31	−62·2 E.
28	M	15 34	23 3	15 10	−74·4 E.
29	Tu	16 47	23 59	11 51	−86·7 E.
30	W	sets	......	7 45	+81·1 M.
31	Th	7 26	0 54	1 7 N	+68·9 M.

SEPTEMBER.					
DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	7 55	1 48	1 43 S	+56·6 M.
2	S	8 23	2 42	6 25	+44·4 M.
3	**S**	8 54	3 36	10 41	+32·2 M.
4	M	9 27	4 31	14 14	+19·9 M.
5	Tu	10 5	5 26	16 53	+ 7·7 M.
6	W	10 51	6 22	18 30	− 4·5 M.
7	Th	11 42	7 17	19 3	−16·7 M.
8	F	12 38	8 10	18 31	−28·9 M.
9	S	13 39	9 2	17 2	−41·1 M.
10	**S**	14 44	9 52	14 44	−53·2 M.
11	M	15 50	10 40	11 46	−65·4 M.
12	Tu	16 55	11 25	8 20	−77·6 M.
13	W	rises	12 9	4 35	−89·8 M.
14	Th	6 51	12 52	0 42 S	+78·1 E.
15	F	7 14	13 35	3 11 N	+65·9 E.
16	S	7 35	14 17	6 55	+53·7 E.
17	**S**	7 59	15 0	10 23	+41·5 E.
18	M	8 27	15 44	13 26	+29·4 E.
19	Tu	8 55	16 30	15 57	+17·2 E.
20	W	9 31	17 18	17 49	+ 5·0 E.
21	Th	10 12	18 8	18 53	− 7·2 E.
22	F	11 2	19 0	19 3	−19·4 E.
23	S	12 1	19 53	18 13	−31·6 E.
24	**S**	13 7	20 47	16 20	−43·9 E.
25	M	14 20	21 42	13 27	−56·1 E.
26	Tu	15 37	22 37	9 40	−68·3 E.
27	W	16 58	23 32	5 11	−80·5 E.
28	Th	sets	……	0 19 N	+87·3 M.
29	F	6 20	0 28	4 37 S	+75·0 M.
30	S	6 51	1 24	9 15 S	+62·8 M.

OCTOBER.					
DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	7 24	2 20	13 14 S	+50·6 M.
2	M	8 2	3 17	16 18	+38·4 M.
3	Tu	8 26	4 15	18 18	+26·2 M.
4	W	9 36	5 11	19 8	+14·0 M.
5	Th	10 32	6 6	18 51	+ 1·8 M.
6	F	11 33	6 59	17 34	−10·4 M.
7	S	12 37	7 50	15 26	−22·6 M.
8	**S**	13 42	8 38	12 36	−34·8 M.
9	M	14 47	9 24	9 16	−46·9 M.
10	Tu	15 51	10 8	5 35	−59·1 M.
11	W	16 54	10 50	1 43 S	−71·3 M.
12	Th	17 59	11 33	2 12 N	−83·4 M.
13	F	rises	12 15	6 1	+84·4 E.
14	S	6 4	12 58	9 35	+72·3 E.
15	**S**	6 29	13 42	12 48	+60·1 E.
16	M	6 57	14 27	15 30	+48·0 E.
17	Tu	7 30	15 14	17 33	+35·8 E.
18	W	8 8	16 3	18 51	+23·6 E.
19	Th	8 53	16 53	19 17	+11·5 E.
20	F	9 48	17 44	18 47	− 0·7 E.
21	S	10 49	18 36	17 18	−12·9 E.
22	**S**	11 58	19 29	14 50	−25·1 E.
23	M	13 10	20 22	11 28	−37·3 E.
24	Tu	14 27	21 15	7 20	−49·5 E.
25	W	15 47	22 10	2 38 N	−61·7 E.
26	Th	17 11	23 5	2 20 S	−73·9 E.
27	F	18 34	……	7 13	−86·1 E.
28	S	sets	0 2	11 39	+81·7 M.
29	**S**	5 54	1 0	15 17	+69·5 M.
30	M	6 37	1 59	17 51	+57·3 M.
31	Tu	7 24	2 59	19 11 S	+45·1 M.

NOVEMBER.					
DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	8 21	3 57	19 17 S	+32·9 M.
2	Th	9 22	4 53	18 15	+20·7 M.
3	F	10 27	5 45	16 17	+ 8·5 M.
4	S	11 31	6 35	13 34	− 3·7 M.
5	**S**	12 37	7 22	10 18	−15·8 M.
6	M	13 42	8 6	6 40	−28·0 M.
7	Tu	14 46	8 49	2 48 S	−40·2 M.
8	W	15 49	9 32	1 8 N	−52·3 M.
9	Th	16 52	10 14	5 1	−64·5 M.
10	F	17 55	10 56	8 43	−76·6 M.
11	S	18 58	11 39	12 5	−88·7 M.
12	**S**	rises	12 25	14 59	+79·1 E.
13	M	5 29	13 11	17 16	+67·0 E.
14	Tu	6 7	14 0	18 48	+54·8 E.
15	W	6 50	14 49	19 29	+42·7 E.
16	Th	7 40	15 40	19 15	+30·5 E.
17	F	8 39	16 31	18 2	+18·4 E.
18	S	9 42	17 22	15 53	+ 6·2 E.
19	**S**	10 53	18 13	12 52	− 5·9 E.
20	M	12 7	19 5	9 4	−18·1 E.
21	Tu	13 21	19 56	4 41 N	−30·3 E.
22	W	14 41	20 49	0 5 S	−42·5 E.
23	Th	16 2	21 44	4 58	−54·7 E.
24	F	17 24	22 40	9 38	−66·8 E.
25	S	18 46	23 39	13 44	−79·0 E
26	**S**	sets	……	16 54	+88·8 M.
27	M	5 8	0 39	18 53	+76·6 M.
28	Tu	6 3	1 40	19 35	+64·4 M.
29	W	7 3	2 39	19 1	+52·2 M.
30	Th	8 8	3 35	17 20 S	+40·0 M.

DECEMBER.					
DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	9 16	4 28	14 47 S	+27·8 M.
2	S	10 23	5 17	11 36	+15·6 M.
3	**S**	11 31	6 3	7 59	+ 3·5 M.
4	M	12 34	6 47	4 7	− 8·7 M.
5	Tu	13 40	7 30	0 8 S	−20·8 M.
6	W	14 43	8 12	3 48 N	−33·0 M.
7	Th	15 46	8 54	7 36	−45·1 M.
8	F	16 49	9 37	11 7	−57·3 M.
9	S	17 51	10 21	14 12	−69·4 M.
10	**S**	18 53	11 8	16 44	−81·5 M.
11	M	rises	11 56	18 33	+86·3 E.
12	Tu	4 47	12 46	19 31	+74·2 E.
13	W	5 36	13 37	19 33	+62·1 E.
14	Th	6 33	14 28	18 36	+49·9 E.
15	F	7 35	15 20	16 41	+37·8 E.
16	S	8 42	16 11	13 52	+25·7 E.
17	**S**	9 54	17 1	10 19	+13·5 E.
18	M	11 6	17 51	6 9	+ 1·4 E.
19	Tu	12 20	18 41	1 35 N	−10·8 E.
20	W	13 40	19 33	3 9 S	−23·0 E.
21	Th	14 58	20 26	7 48	−35·1 E.
22	F	16 19	21 22	12 3	−47·3 E.
23	S	17 37	22 20	15 36	−59·5 E.
24	**S**	18 51	23 20	18 9	−71·7 E.
25	M	sets	……	19 29	−83·9 E.
26	Tu	4 41	0 20	19 31	+83·9 M.
27	W	5 45	1 18	18 20	+71·8 M.
28	Th	6 53	2 14	16 7	+59·6 M.
29	F	8 3	3 6	13 6	+47·4 M.
30	S	9 13	3 55	9 33	+35·2 M.
31	**S**	10 21	4 42	5 40 S	+23·0 M.

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. DENNING.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	a °	δ °			*a* °	δ °	
Jan. 2-3. .	**230**	**+53**	Swift; long paths.	July-Sept. .	335	+73	Swift; short.
3 ...	156	+41	Swift.	July-Aug. .	339	−27	Slow; long.
11 ...	220	+13	Swift; streaks.	July-Oct. 8	30	+36	Swift; streaks.
17 ...	295	+53	Slow; bright.	Aug. 4-10	350	+49	Swift.
17-23	159	+27	Swift.	10-12	**45**	**+57**	Swift; streaks.
25 ...	131	+32	Swift.	10-Sept. 16	353	−11	Rather slow.
29 ...	213	+52	Very swift.	Aug. 15 ...	290	+53	Swift; bright.
Feb. 5-10	75	+41	Slow; bright.	21-25	291	+60	Slow; bright.
15 ...	236	+11	Swift; streaks.	25 ...	5	+11	Slow; short.
15 ...	261	+ 4	Swift; streaks.	Aug.-Sept. .	346	± 0	Slow.
20 ...	181	+34	Swift; bright.	Aug.-Oct. 2	74	+42	Swift; streaks.
20 ...	263	+36	Swift; streaks.	Sept. 3-8 .	353	+39	Very swift.
Mar. 1-4...	166	+ 4	Slow; bright.	5-15	62	+37	Swift; streaks.
14 ...	250	+54	Swift.	6-17	106	+52	Swift; streaks.
18 ..	316	+76	Slow; bright.	15 ...	77	+57	Swift; streaks.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow; trains.
27 ...	229	+32	Swift; small.	27 ...	75	+15	Swift; streaks.
Mar.-Ap.-May	263	+62	Rather swift.	Oct. 2 ...	230	+52	Slow; bright.
Apr. 12-24	210	−10	Slow; fireballs.	4 ...	133	+79	Swift; streaks.
Ap. 17-May 1	240	+47	Small; short.	4 ...	310	+79	Slowish.
Apr. 18-23	189	−31	Slow; long.	8 ...	77	+31	Swift; streaks.
20-21	261	+36	Swift; bl. white.	8-14	45	+58	Small; short.
20-22	**271**	**+33**	Swift.	14 ...	133	+68	Rather swift.
20-25	218	−31	Slow; long paths.	16-23	89	+ 8	Swift; streaks.
30 ...	291	+59	Rather slow.	18-20	**92**	**+15**	Swift; streaks.
Apr.-May .	193	+58	Slow; yellow.	23 ...	99	+13	Swift; streaks.
May 1-6 .	**338**	**− 2**	Swift; streaks.	29 ...	109	+23	Very swift.
7 ...	246	+ 3	Slow; bright.	Nov. 1 ...	43	+22	Slow; bright.
11-18	231	+27	Slow; small.	2 ...	55	+ 9	Slow; bright.
30-Aug.	333	+27	Swift; streaks.	10-12	133	+31	Very swift; streaks.
May-June .	235	+ 9	Rather slow.	14-16	**150**	**+22**	Swift; streaks.
May-July .	252	−21	Slow; trains.	16-28	154	+41.	Swift; streaks.
June 10 ...	261	+ 5	Very slow.	20-23	63	+23	Slow; bright.
11-19	274	+69	Rather swift.	17-23	**25**	**+43**	Very slow; trains.
13 ...	310	+61	Swift; streaks.	23-Dec. 12	189	+73	Rather swift.
June-Sep. . .	335	+57	Swift; slow in Sep.	Nov. 30 ...	190	+58	Swift; streaks.
June 26 ...	354	+39	Swift; streaks.	Dec. 4 ...	162	+58	Swift; streaks.
June-Aug. .	303	+24	Swift.	6 ...	80	+23	Slow; bright.
July 6-22	284	−13	Very slow.	8 ...	145	+ 7	Swift; streaks.
15-31	23	+43	Swift; streaks.	8 ...	208	+71	Rather swift.
19 ...	315	+48	Swift; short.	10-12	**108**	**+33**	Swift; short.
July-Aug. .	269	+48	Slow; trains.	12 ...	119	+29	Rather swift.
9-Sep. 18	323	−24	Slow; long.	22 ...	194	+67	Swift; streaks.
25-Sep. 15	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.
July 28 ...	**339**	**−11**	Slow; long.	31 ...	92	+57	Slow; bright.

The radiant-points of the more brilliant showers are indicated by heavier type. The Andromedids of November may be very plentiful in 1905, November 17–18.

The Perseids, with max. on August 11, are visible for a considerable period and their radiant-point exhibits a motion to E.N.E. amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	a °	δ °		*a* °	δ °		*a* °	δ °
July 19 ...	18·9	+50·5	July 29 ...	29·3	+53·8	Aug. 8 ...	41·5	+56·5
21 ...	20·8	+51·1	31 ...	31·6	+54·4	10 ...	44·3	+56·9
23 ...	22·8	+51·8	Aug. 2 ...	33·9	+55·0	12 ...	47·1	+57·3
25 ...	24·9	+52·5	4 ...	36·4	+55·5	14 ...	50·0	+57·7
27 ...	27·1	+53·2	6 ...	38·9	+56·0	16 ...	52·9	+58·0

1505.	Mercury (☿). R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	Venus (♀). R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	Mars (♂). R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1..	18 35 57	20 25 S.	10·0	19 33	23 44	3 55	21 43 5	15 34 S.	16·6	22 19	3 1	7 43	13 30 5	7 37 S.	6·0	13 22	18 47	0 12
11..	18 3 43	20 29	8·4	18 26	22 39	2 50	22 27 31	11 5	17·8	21 59	3 6	8 13	13 49 40	9 28	6·4	13 12	18 27	23 42
21...	18 27 37	21 52	6·8	18 24	22 27	2 30	23 9 27	6 11	19·2	21 36	3 9	8 42	14 8 33	11 9	6·8	13 0	18 6	23 12
31...	19 17 38	22 22	5·8	18 39	22 39	2 39	23 49 8	1 5 S.	20·8	21 11	3 9	9 7	14 26 35	12 40	7·4	12 48	17 45	22 42
Feb. 10..	20 17 44	21 2	5·2	18 51	23 0	3 9	0 26 46	4 0 N.	23·0	20 43	3 7	9 31	14 43 24	14 0	8·0	12 32	17 22	22 12
20..	21 22 6	17 32	5·0	18 55	23 26	3 57	1 2 14	8 53	25·6	20 14	3 3	9 52	14 58 40	15 9	8·8	12 14	16 58	21 42
Mar. 2...	22 28 53	11 46	4·8	18 49	23 53	4 57	1 35 0	13 22	29·0	19 43	2 56	10 9	15 11 53	16 6	9·6	11 54	16 32	21 10
12...	23 38 5	3 51 S.	5·0	18 35	0 20	6 5	2 3 51	17 16	33·2	19 10	2 46	10 22	15 22 24	16 50	10·6	11 28	16 2	20 36
22...	0 48 7	5 26 N.	5·6	18 19	0 51	7 23	2 26 23	20 21	38·4	18 34	2 29	10 24	15 29 30	17 23	11·8	11 0	15 30	20 0
Apr. 1...	1 47 12	13 24	7·0	17 56	1 10	8 24	2 39 0	22 18	45·0	17 55	2 2	10 9	15 32 23	17 43	13·0	10 25	14 53	19 21
11...	2 15 19	16 53	9·2	17 26	0 59	8 32	2 37 47	22 40	52·2	17 11	1 21	9 31	15 30 17	17 49	14·4	9 43	14 11	18 39
21..	2 6 41	14 52	11·4	16 51	0 11	7 31	2 22 1	20 52	57·8	16 29	0 26	8 23	15 23 0	17 42	15·8	8 57	13 25	17 53
May 1...	1 46 58	10 11	11·6	16 12	23 7	6 2	2 0 10	17 11	58·2	15 45	23 19	6 53	15 11 5	17 21	16·8	8 3	12 33	17 3
11...	1 47 49	8 4	10·0	15 47	22 31	5 15	1 45 45	13 21	53·4	15 14	22 27	5 40	14 56 24	16 48	17·4	7 5	11 39	16 13
21..	2 13 43	9 48	8·2	15 26	22 20	5 14	1 45 26	11 1	46·0	14 49	21 49	4 49	14 41 51	16 13	17·4	6 9	10 46	15 23
31...	2 59 54	14 10	6·6	15 9	22 28	5 47	1 58 11	10 30	39·2	14 26	21 23	4 20	14 30 12	15 46	16·8	5 15	9 55	14 35
June 10...	4 5 42	19 35	5·6	15 5	22 57	6 49	2 20 31	11 21	33·4	14 5	21 7	4 9	14 23 20	15 36	16·0	4 28	9 9	13 50
20...	5 32 9	23 52	5·2	15 24	23 46	8 8	2 49 39	13 4	29·0	13 45	20 57	4 9	14 21 57	15 48	15·0	3 48	8 28	13 8
30...	7 7 12	24 18	5·2	16 11	0 36	9 1	3 23 52	15 11	25·6	13 28	20 52	4 16	14 25 48	16 22	13·8	3 17	7 53	12 29
July 10...	8 29 59	20 46	5·6	17 20	1 19	9 18	4 2 8	17 21	22·8	13 14	20 51	4 28	14 34 21	17 13	12·8	2 51	7 22	11 53
20..	9 34 14	15 15	6·2	18 19	1 44	9 9	4 43 45	19 15	20·6	13 47	20 54	4 41	14 46 58	18 19	12·0	2 31	6 56	11 21
30...	10 21 12	9 22	7·2	18 59	1 51	8 43	5 28 14	20 40	18·8	13 1	20 59	4 57	15 3 2	19 33	11·2	2 15	6 33	10 51
Aug. 9...	10 49 42	4 28	8·6	19 14	1 40	8 6	6 14 55	21 21	17·4	13 4	21 6	5 8	15 22 7	20 50	10·4	2 3	6 12	10 21
19...	10 52 49	2 24	10·2	18 49	1 4	7 19	7 3 3	21 12	16·0	13 14	21 15	5 16	15 43 50	22 6	9·8	1 54	5 55	9 56
29...	10 27 15	5 13	10·6	17 24	23 52	6 20	7 51 51	20 7	15·0	13 31	21 25	5 19	16 7 51	23 15	9·2	1 45	5 39	9 33
Sept. 8...	10 8 35	9 59	8·8	16 4	22 58	5 52	8 40 32	18 7	14·2	13 52	21 34	5 16	16 33 55	24 15	8·8	1 39	5 26	9 13
18..	10 38 41	9 49	6·6	15 58	22 53	5 48	9 28 34	15 16	13·4	14 17	21 42	5 5	17 1 46	24 59	8·4	1 35	5 15	8 55
28..	11 40 3	4 12 N.	5·4	16 50	23 16	5 42	10 15 42	11 41	12·8	14 45	21 50	4 55	17 31 4	25 26	8·0	1 27	5 4	8 41
Oct. 8...	12 44 59	3 21 S.	4·8	17 54	23 41	5 28	11 1 56	7 31	12·4	15 15	21 57	4 39	18 1 32	25 31	7·6	1 19	4 56	8 33
18...	13 46 59	10 41	4·6	18 53	0 2	5 11	11 47 34	2 57 N.	11·8	15 44	22 3	4 22	18 32 48	25 13	7·2	1 8	4 47	8 26
28..	14 47 35	16 58	4·8	19 49	0 23	4 57	12 33 5	1 49 S.	11·6	16 14	22 9	4 4	19 4 31	24 30	7·0	0 57	4 40	8 23
Nov. 7...	15 48 30	21 50	5·0	20 41	0 45	4 49	13 19 1	6 36	11·2	16 45	22 16	3 47	19 36 23	23 23	6·6	0 39	4 32	8 25
17...	16 49 11	24 54	5·6	21 24	1 6	4 48	14 5 59	11 10	10·8	17 16	22 23	3 30	20 8 4	21 51	6·4	0 21	4 24	8 27
27...	17 42 58	25 46	6·6	21 45	1 20	4 55	14 54 30	15 19	10·6	17 48	22 32	3 16	20 39 22	19 57	6·2	0 1	4 16	8 31
Dec. 7...	18 5 4	24 14	8·6	21 16	1 2	4 48	15 44 55	18 49	10·4	18 22	22 44	3 6	21 10 7	17 44	5·8	23 39	4 8	8 37
17...	17 22 11	20 52	9·8	19 23	23 31	3 39	16 37 15	21 28	10·2	18 51	22 57	3 3	21 40 12	15 13	5·6	23 14	3 58	8 42
27...	16 56 48	19 46 S.	8·0	18 19	22 33	2 49	17 31 12	23 4 S.	10·2	19 15	23 11	3 7	22 9 39	12 29 S.	5·4	22 49	3 48	8 47

Mercury.

	d h	d h	d h
Gr. elong. E.	Apr. 4 2;	Aug. 2 1;	Nov. 26 17
„ W.	Jan. 22 6;	May 20 23;	Sept. 14 23
Sup. conj. ...	Mar. 9 17;	June 23 22;	Oct. 11 20
Inf. conj. ...	Apr. 23 9;	Aug. 29 15;	Dec. 15 10
Stationary...	Jan. 10 22;	Apr. 13 4;	May 5 17
	Aug. 15 3;	Sept. 7 8;	Dec. 5 19
			Dec. 25 15

Venus.

	d h	d h
Gr. elong. E.	Feb. 14 12	
„ W.	July 6 1	
Inf. conj. ...	Apr. 26 22	
Stationary...	Apr. 5 6;	May 16 4
Gr. brilliancy	Mar. 21 12;	June 2 2

Superior Planets.

	Conjunction. d h	Opposition. d h	Quadrature. d h	d h	Stationary. d h	d h
Mars......	...	May 8 8	Jan. 26 12;	Aug. 26 5	Apr. 1 23;	June 17 13
Jupiter ..	May 3 18	Nov. 23 21	Jan. 11 20;	Aug. 28 20	Sept. 25 9	
Saturn ...	Feb. 12 8	Aug. 22 21	May 23 23;	Nov. 19 0	June 14 0;	Oct. 31 6
Uranus...	Dec. 26 7	June 23 23	Mar. 24 23 Sept. 23 14	....	Apr. 8 9;	Sept. 9 4
Neptune .	June 29 21	Dec. 30 20	Mar. 26 5;	Oct. 3 19	Mar. 16 15:	Oct. 13 17

Jupiter (♃). **Saturn (♄).**

1905.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1	1 18 29	6 54 N.	38·8	23 59	6 36	13 13	21 25 36	16 20 S.	14·4	22 6	2 43	7 20
21	1 24 54	7 39	36·4	22 41	5 23	12 5	21 34 19	15 38	14·2	20 52	1 33	6 14
Feb. 10	1 35 24	8 46	34·2	21 27	4 15	11 3	21 43 39	14 53	14·2	19 39	0 24	5 9
Mar. 2	1 49 4	10 8	32·6	20 15	3 10	10 5	21 53 1	14 6	14·2	18 20	23 11	4 2
22	2 5 4	11 39	31·4	19 3	2 7	9 10	22 1 50	13 22	14·4	17 7	22 1	2 55
Apr. 11	2 22 39	13 12	30·8	17 54	1 6	8 18	22 9 33	12 43	14·8	15 53	20 50	1 47
May 1	2 41 9	14 43	30·4	16 46	0 6	7 26	22 15 40	12 12	15·2	14 38	19 38	0 38
21	2 59 57	16 7	30·4	15 35	23 3	6 31	22 19 45	11 54	15·6	13 22	18 23	23 24
June 10	3 18 27	17 22	31·0	14 27	22 3	5 39	22 21 30	11 48	16·2	12 4	17 6	22 8
30	3 35 57	18 25	32·0	13 20	21 2	4 44	22 20 46	11 57	16·8	10 46	15 47	20 48
July 20	3 51 42	19 15	33·2	12 12	19 59	3 46	22 17 42	12 18	17·2	9 26	14 25	19 24
Aug. 9	4 4 48	19 52	35·0	11 2	18 53	2 44	22 12 51	12 49	17·4	8 6	13 1	17 56
29	4 14 13	20 15	37·2	9 50	17 44	1 38	22 7 9	13 22	17·4	6 44	11 37	16 30
Sept. 18	4 18 56	20 25	39·6	8 34	16 29	0 24	22 1 46	13 51	17·4	5 22	10 13	15 4
Oct. 8	4 18 11	20 21	42·0	7 16	15 10	23 4	21 57 49	14 12	17·0	4 1	8 50	13 39
28	4 12 3	20 5	44·0	5 53	13 45	21 37	21 56 5	14 19	16·4	2 42	7 30	12 18
Nov. 17	4 1 57	19 39	45·0	4 26	12 16	20 6	21 57 0	14 12	15·8	1 23	6 12	11 1
Dec. 7	3 50 46	19 8	44·6	3 1	10 46	18 31	22 0 29	13 51	15·4	0 6	4 57	9 48
27	3 41 51	18 44 N.	43·2	1 35	9 19	17 3	22 6 14	13 19 S.	15·0	22 50	3 44	8 38

Uranus (♅). **Neptune (♆).**

Jan. 1	18 3 8	23 39 S.	3·3	19 27	23 18	3 9	6 28 52	22 15 N.	2·7	3 38	11 45	19 52
31	18 10 28	23 39	3·4	17 36	21 27	1 18	6 25 31	22 18	2·7	1 37	9 44	13 51
Mar. 2	18 15 54	23 38	3·5	15 44	19 34	23 24	6 23 30	22 20	2·7	23 37	7 44	15 51
Apr. 1	18 18 24	23 37	3·6	13 49	17 39	21 29	6 23 32	22 22	2·6	21 39	5 46	13 53
May 1	18 17 35	23 38	3·7	11 50	15 40	19 30	6 25 40	22 22	2·6	19 43	3 50	11 57
31	18 13 55	23 40	3·7	9 48	13 38	17 28	6 29 27	22 20	2·5	17 49	1 56	10 4
June 30	18 8 45	23 42	3·8	7 45	11 35	15 25	6 34 7	22 17	2·5	15 52	23 59	8 6
July 30	18 3 59	23 43	3·7	5 43	9 33	13 23	6 38 30	22 13	2·5	13 58	22 5	6 13
Aug. 29	18 1 18	23 43	3·6	3 42	7 32	11 22	6 42 34	22 9	2·6	12 5	20 11	4 17
Sept. 28	18 1 44	23 43	3·5	1 44	5 34	9 24	6 44 42	22 6	2·6	10 9	18 15	2 21
Oct. 28	18 5 24	23 42	3·5	23 50	3 40	7 30	6 44 45	22 5	2·6	8 11	16 17	0 23
Nov. 27	18 11 38	23 41	3·4	21 58	1 48	5 38	6 42 47	22 7	2·7	6 11	14 17	22 23
Dec. 27	19 19 17	23 38 S.	3·3	20 5	23 55	3 45	6 39 26	22 10 N.	2·7	4 10	12 16	20 22

Ceres. **Pallas.**

1905.	R.A. Noon.	Dec. Noon.	Transits.	1905.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m		h m	° ′	h m
Aug. 21 ...	23 29	20 8 S.	13 30	July 16 ...	20 35	17 21 N.	12 58
Sept. 2 ...	23 20	21 26	12 34	28 ...	20 26	16 21	12 1
14 ...	23 10	22 27	11 37	Aug. 9 ...	20 16	14 48	11 5
26 ...	23 1	23 2 S.	10 40	21 ...	20 8	12 47	10 10
				Sept. 2 ...	20 2	10 28 N.	9 16

Juno. **Vesta.**

Mar. 6 ...	0 24	1 23 S.	1 30	Mar. 6 ...	12 46	7 50 N.	13 49
18 ...	0 50	0 44 N.	1 8	18 ...	12 37	9 25	12 53
30 ...	1 15	2 50	0 46	30 ...	12 26	10 49	11 55
Apr. 11 ...	1 42	4 53 N.	0 25	Apr. 11 ...	12 16	11 47 N.	10 57

The times of rising and setting correspond to the adjacent transit, irrespective of the day given in the first column.

ECLIPSES IN 1905.

In the year 1905 there will be two Eclipses of the Sun and two of the Moon.

I.—*A Partial Eclipse of the Moon*, 1905, *February* 19.

	h m		h m
First contact with Penumbra	4 41·3	With shadow	5 53·7
Last „ „	9 19·1	„ „	8 6·7
Middle of Eclipse..................	7 0·2		

Visible at Greenwich. Magnitude of the Eclipse (Moon's diam. = 1) 0·410. First contact with shadow is at 54° from N. point towards E. Last contact 336° towards the E. The Moon will rise at Greenwich at $5^h\ 16^m$ obscured by the Penumbra.

II.—*An Annular Eclipse of the Sun*, 1905, *March* 5.

Invisible at Greenwich. The Eclipse will be visible only from Australia and the islands in the Southern Ocean. The Line of Central Eclipse passes through Queensland and South Australia. A Partial Eclipse will be visible at Perth, Adelaide, Melbourne, and Sydney; the magnitude of the Eclipse at these places being respectively 0·787, 0·827, 0·724, and 0·725 (Sun's diam. = 1).

III.—*A Partial Eclipse of the Moon*, 1905, *August* 14.

Visible at Greenwich. Magnitude of the Eclipse (Moon's diam. = 1) 0·292. The first contact with the shadow will occur at 134° from the N. point towards the E. Last contact 200° towards the East.

	h m		h m
First contact with Penumbra	13 9·5	With shadow	14 38·9
Last „ „	18 12·5	„ „	16 43·1
Middle of Eclipse	15 41·0		

The Moon will set at Greenwich at $16^h\ 53^m$.

IV.—*A Total Eclipse of the Sun*, 1905, *August* 29–30.

Invisible at Greenwich. The centre of the shadow begins its course in the Dominion of Canada west of Hudson's Bay; leaving the southern part of the Coast of Labrador it crosses the Atlantic and strikes land again on the north-west point of Spain, near Gijon. Thence it crosses Tunis, the Libyan Desert, Assouan, the Red Sea, and ends its course in Arabia, where the Sun will be setting.

	Eclipse begins.	Totality begins.	Totality ends.	Sun's Altitude.
	d h m	d h m	d h m	°
Labrador............	29 22 51·4	29 23 54·2	29 23 56·8	27
Spain N.W.	29 23 40·9	30 1 0·6	30 1 4·4	56
Spain E.............	29 23 55·9	30 1 16·5	30 1 20·3	55
Tripoli N.	30 0 34·8	30 1 52·0	30 1 55·4	44
Assouan	30 1 14·9	30 2 21·8	30 2 24·4	24

Central Eclipse begins August $29^d\ 22^h\ 37^m·7$ G.M.T., in longitude 76° 18′ W., lat. 37° 31′ N.

Central Eclipse ends August $30^d\ 3^h\ 37^m·3$ G.M.T., in longitude 36° 45′ E., lat. 5° 44′ N.

A Partial Eclipse will be visible throughout the British Isles:—

	Eclipse begins.	Greatest Eclipse.	Eclipse ends.	Magnitude.
	d h m	d h m	d h m	
Greenwich	29 23 49	30 1 4	30 2 15	0·786
Edinburgh	23 44	0 55	2 4	0·724
Dublin..................	23 39	0 53	2 5	0·799
Montreal............	22 40	29 23 38	0 40	0·811

The times are Greenwich Mean in all cases.

OCCULTATIONS, 1905. (VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap.		Reap.	
			M. T.	P.	M. T.	P.
Jan.			h m	°	h m	°
10	ϕ Aquarii ……	4·4	5 9	45	6 23	265
14	B.A.C. 764……	6·3	12 50	99	13 43‡	236
16	D.M.+14° 657.	6·5	11 54	93	13 0	251
16	48 Tauri……	6·4	15 45	86	16 36†	264
17	B.A.C. 1526 …	5·8	9 40	37	10 36	306
18	130 Tauri ……	5·5	5 58	103	7 1	240
19	26 Geminorum .	5·1	4 51	140	5 24	213
19	D.M.+17° 1479	6·2	14 48	80	15 47	296
20	B.A.C. 2649 …	6·3	12 56	88	14 5	296
20	5 Cancri ………	6·4	14 34	66	15 26	320
22	R Leonis………	var.	8 7	57	8 48	327
23	56 Leonis ……	6·5	13 38	134	14 45	271
24	β Virginis ……	3·8	12 43	145	13 40	260
25	38 Virginis……	6·2	16 44	120	17 57	290
28	γ Libræ ………	4·1	15 7	91	16 11	307
Feb.						
13	71 Tauri………	4·6	3 48	101	4 54	228
13	θ^2 Tauri ………	3·6	5 12	78	6 32	254
13	θ^1 Tauri ………	3·9	5 14	56	6 29	275
13	D.M.+15° 633.	6·5	6 6	357	6 23	334
13	B.A.C. 1391 …	4·9	6 48	24	7 37	311
13	85 Tauri………	6·5	7 23	127	8 17	212
13	89 Tauri………	6·5	11 10	133	11 54	218
14	111 Tauri ……	5·2	6 27	99	7 42	244
15	D.M.+17° 1214	6·5	7 15	153	7 47	202
15	B.A.C. 2116 …	6·5	12 57	114	13 54	257
16	D.M.+17° 1596	5·6	13 26	91	14 27	290
17	D.M.+16° 1679	6·5	6 36	118	7 38	255
19	44 Leonis ……	6·2	10 29	86	11 33	313
20	89 Leonis ……	5·7	16 20	108	17 22	297
21	η Virginis ……	4·0	10 5	166	10 40	237
22	l^1 Virginis ……	6·1	17 3	92	18 8	312
28	D.M.−19° 5273	5·9	15 30†	150	16 6	217
28	B.A.C. 6536 …	5·5	18 25	159	18 52	199
Mar.						
12	48 Tauri………	6·4	7 55	89	9 6	256
12	γ Tauri ………	3·9	10 11	62	11 6	288
14	D.M.+17° 1182	6·5	14 19‡	57	14 59†	309
15	D.M+17° 1479.	6·2	10 47	73	11 47	303
16	B.A.C. 2649 …	6·3	10 5	76	11 9	308
16	5 Cancri………	6·4	11 47	48	12 24	338
18	R Leonis ……	var.	5 48	68	6 40	319
19	56 Leonis ……	6·5	11 2	130	12 10	276
20	β Virginis ……	3·8	9 2	156	9 49	249
21	38 Virginis……	6.2	11 10	153	12 3	257
21	k Virginis ……	5·9	14 35	49	15 5	357
22	B.A.C. 4680 …	6·5	17 40	106	18 41	287
24	49 Libræ ……	5·6	17 27	168	17 57	217
29	B.A.C. 7097 …	6·2	15 42	77	16 53	270
Apr.						
8	D.M.+14° 657.	6·5	10 23‡	61	11 11†	288
10	130 Tauri ……	5·5	8 57	127	9 48	237
12	D.M.+17° 1596	5·6	6 37	182	6 45	193
Apr.			h m	°	h m	°
15	44 Leonis ……	6·2	8 3	101	9 14	303
16	89 Leonis ……	5·7	14 33	105	15 29	296
17	η Virginis ……	4·0	8 18	150	9 12	258
18	l^1 Virginis ……	6·1	14 45	68	15 31	332
24	B.A.C. 6616 …	6·5	13 9	139	13 55	222
May						
6	α Tauri ………	1·1	5 28	63	6 30	287
12	A Leonis………	4·6	8 46	124	9 53	279
13	c Leonis ………	5·1	7 26	136	8 34	272
15	38 Virginis……	6·2	8 30	191	8 48	220
15	k Virginis ……	5·9	11 30	76	12 24	329
16	B.A.C. 4680 …	6·5	15 13	107	16 10†	285
18	49 Libræ………	5·6	13 51	110	15 1	274
19	29 Ophiuchi …	6·5	14 17	126	15 20	247
27	29 Piscium ……	5·1	13 42†	103	14 35	221
June						
12	l^1 Virginis ……	6·1	7 56	166	8 41	244
12	l^2 Virginis ……	4·9	8 22	83	9 24	326
18	B.A.C. 6707 …	6·2	15 45	16	16 18	324
20	29 Capricorni…	5·5	9 53†	63	10 52	280
25	f Piscium ……	5·3	14 21	3	14 48	314
29	θ^2 Tauri ………	3·6	14 10†	64	15 1	270
29	θ^1 Tauri ………	3·9	14 15†	41	14 56	294
29	80 Tauri………	5·7	15 2	156	15 12	177
July						
15	B.A.C. 6550 …	6·5	12 53	122	13 51	224
25	D.M.+12° 473.	6·2	15 28	65	16 41	254
Aug.						
6	B.A.C. 4666 …	6·5	8 45	44	9 13	351
17	27 Piscium ……	5·0	11 59	57	13 17	253
17	29 Piscium ……	5·1	14 40	23	15 41	285
23	89 Tauri………	6·5	11 31	78	12 26	256
23	σ^2 Tauri ………	4·8	12 5	121	12 46	211
24	117 Tauri ……	6·5	11 6†	146	11 28†	196
Sept.						
4	γ Libræ ………	4·1	7 33	85	8 37	301
8	B.A.C. 6616 …	6·5	5 56	39	6 45	319
10	29 Capricorni…	5·5	10 21	37	11 27	287
15	33 Ceti ………	6·3	8 35	89	9 37	228
15	f Piscium ……	5·3	14 17	6	15 0	302
17	μ Ceti…………	4·4	10 35	42	11 36	275
18	f Tauri ………	4·3	9 54	65	10 55	259
19	48 Tauri………	6·4	8 49†	358	8 59*	335
19	γ Tauri ………	3·9	10 36	8	10 59	322
19	71 Tauri………	4·6	13 56	132	14 37	195
19	θ^1 Tauri ………	3·9	15 13	81	16 36	249
19	θ^2 Tauri ………	3·6	15 18	104	16 31	226
19	D.M.+15° 633.	6·5	15 44	42	16 54	289
19	B.A.C. 1391 …	4·9	16 42	57	18 0	278
19	α Tauri ………	1·1	20 24	26	21 1	322
21	D.M.+18° 1129	6·5	16 37	26	17 18	324
23	3 Cancri………	6·0	15 12	42	15 54	322

† Star below the horizon. ‡ Star setting. * Star rising.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

Star.	Mag.	Disap. M.T.	Disap. P.	Reap. M.T.	Reap. P.
		h m	°	h m	°
B.A.C. 6550 ...	6·5	7 12	103	8 24	244
D.M.+14° 657.	6·5	10 54	62	12 7	261
48 Tauri	6·4	16 43	160	16 56	179
B.A.C. 1526 ...	5·8	10 59	60	12 7	271
f Geminorum...	5·2	13 56	149	14 34	214
45 Leonis	5·9	15 41	73	16 35	319
ρ Leonis	3·8	18 24	85	19 30	317
29 Sagittarii ...	5·5	6 51	140	7 29	207
57 Sagittarii ...	6·2	9 11	103	10 6†	232
42 Capricorni...	5·3	10 55	89	11 55†	232
σ Aquarii	4·8	10 59	75	12 4	240
27 Piscium	5·0	7 30	35	8 42	273
29 Piscium	5·1	10 26	352	10 52	315
4 Ceti	6·4	13 46	51	14 46‡	267
5 Ceti	6·3	14 8	38	15 0†	281
ν Piscium	4·7	15 52	73	16 51†	253
D.M.+14° 657.	6·5	18 56	114	19 44	232
α Tauri	1·1	6 35	32	7 16	299
B.A.C. 1526 ...	5·8	19 0	120	19 49	234

Date.	Star.	Mag.	Disap. M.T.	Disap. P.	Reap. M.T.	Reap. P.
Nov.			h m	°	h m	°
14	115 Tauri	5·4	6 49	75	7 44	264
23	*n* Virginis	7·0	15 39†	63	16 15	339
Dec.						
5	14 Ceti	6·0	10 38	77	11 47	236
6	33 Ceti	6·3	3 37	36	4 35	279
8	μ Ceti	4·4	5 35	52	6 43	263
9	*f* Tauri	4·3	4 49	101	5 43	220
10	48 Tauri	6·4	3 8†	80	4 1	251
10	γ Tauri	3·9	4 58	84	5 56	244
10	75 Tauri	5·3	10 3	60	11 22	269
10	D.M.+15° 633.	6·5	10 24	115	11 26	215
10	B.A.C. 1391 ...	4·9	11 37	137	12 18	197
10	α Tauri	1·1	14 52	109	15 53	238
12	71 Orionis	5·1	11 52	48	12 57	304
15	o^2 Cancri.........	5·6	12 27	114	13 40	267
19	γ Virginis (mean)	3·0	19 53	133	21 2	278
20	65 Virginis......	6·1	13 16†	161	13 49	239
29	44 Capricorni...	5·8	3 53	30	4 53	289
29	45 Capricorni...	5·8	4 22	138	4 49	180

† Star below the horizon. ‡ Star setting.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

ollowing "near approaches" are also given in the 'Nautical Almanac,' and are especially iteresting to observers rather South and rather North of Greenwich respectively :—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
			h m	°				h m	°
Jan. 19	D.M.+18° 1349	6·5	7 26	357	June 29	α Tauri	1·1	18 12	344
22	A Leonis	4·6	18 48	19	July 15	B.A.C. 6536 ...	5·5	12 39	354
23	*c* Leonis	5·1	16 34	23	17	B.A.C. 7263 ...	6·0	11 37	348
Feb. 28	D.M.−18° 5206	6·5	17 59	0	26	D.M.+14° 657	6·5	13 12	345
Mar. 19	*c* Leonis	5·1	13 50	23	Aug. 16	*h′* Aquarii	5·5	8 29	344
22	B.A.C. 4647 ...	6·4	12 54	24	Sept. 11	e^2 Aquarii	5·4	14 4	338
Apr. 9	B.A.C. 1526 ...	5·8	10 26	358	Oct. 7	B.A.C. 7263 ...	6·0	7 43	345
11	D.M.+18° 1349	6·5	12 3	6	11	4 Ceti	6·4	6 16	340
13	B.A.C. 2888 ...	6·5	10 57	16	11	5 Ceti	6·3	6 37	339
May 16	B.A.C. 4647 ...	6·4	10 37	23	13	ν Piscium	4·7	8 15	338
21	D.M.−19° 5273	5·9	15 56	355	Nov. 14	120 Tauri	5·3	10 37	348
June 14	γ Libræ	4·1	13 49	11	Dec. 11	119 Tauri	4·6	18 2	0
21	e^2 Aquarii	5·4	13 3	345	11	120 Tauri	5·3	18 38	0
29	D.M.+15° 633	6·5	14 51	347	14	ζ Cancri	4·7	17 58	14
29	B.A.C. 1391 ...	4·9	15 31	346	16	ν Leonis	5·2	17 11	21

South Near Approaches.

			h m	°				h m	°
Feb. 13	80 Tauri	5·7	6 38	167	Sept. 14	14 Ceti	6·0	16 17	157
13	81 Tauri	5·5	7 0	168	19	85 Tauri	6·5	18 1	169
13	σ^2 Tauri	4·8	12 5	176	Oct. 15	D.M.+12° 473	6·2	14 36	162
14	117 Tauri	6·5	9 15	176	15	*f* Tauri	4·3	18 54	170
Mar. 11	D.M.+12° 473	6·2	7 10	167	Nov. 23	B.A.C. 4647 ...	6·4	19 35	205
11	*f* Tauri	4·3	10 50	171	Dec. 10	θ′ Tauri	3·9	10 30	165
Apr. 11	26 Geminorum.	5·1	9 57	187	11	115 Tauri	5·4	15 26	178
17	13 Virginis......	6·3	8 16	204	12	D.M.+18° 1129	6·5	11 30	174
May 10	D.M.+16° 1679	6·5	9 41	194	15	*o′* Cancri.........	5·2	12 59	190
Aug. 15	σ Aquarii	4·8	16 53	158	20	66 Virginis......	5·8	14 11	201
23	σ′ Tauri	5·1	12 19	166					

JUPITER'S SATELLITES, 1905.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re.; *t*, Tr. Ing.; T, Tr. Egr.

January.

d	h	m	
1	9	51	i. E.
	18	48	i. *t*.
	21	1	i. T.
2	1	6	ii. *o*.
	3	38	ii. O.
	3	49	ii. *e*.
	6	13	ii. E.
	14	51	iii. *o*.
	16	8	i. *o*.
	17	3	iii. O.
	19	39	i. E.
	20	33	iii. *e*.
	22	8	iii. E.
3	13	16	i. *t*.
	15	30	i. T.
	20	12	ii. *t*.
	22	43	ii. T.
4	10	37	i. *o*.
	14	8	i. E.
5	7	45	i. *t*.
	9	59	i. T.
	14	23	ii. *o*.
	16	55	ii. O.
	17	7	ii. *e*.
	19	32	ii. E.
6	4	52	iii. *t*.
	5	5	i. *o*.
	7	5	iii. T.
	8	37	i. E.
7	2	14	i. *t*.
	4	27	i. T.
	9	29	ii. *t*.
	12	1	ii. T.
	23	34	i. *o*.
8	3	6	i. E.
	20	42	i. *t*.
	22	56	i. T.
9	3	41	ii. *o*.
	6	13	ii. O.
	6	26	ii. *e*.
	8	50	ii. E.
	18	3	i. *o*.
	18	48	iii. *o*.
	21	2	iii. O.
	21	35	i. E.
10	0	36	iii. *e*.
	2	10	iii. E.
	15	11	i. *t*.
	17	25	i. T.
	22	48	ii. *t*.
11	1	20	ii. T.
	12	32	i. *o*.
	16	4	i. E.
12	9	40	i. *t*.

Jan. (con.).

d	h	m	
12	11	54	i. T.
	16	59	ii. *o*.
	19	31	ii. O.
	19	44	ii. *e*.
	22	9	ii. E.
13	7	1	i. *o*.
	8	52	iii. *t*.
	10	32	i. E.
	11	6	iii. T.
14	4	9	i. *t*.
	6	23	i. T.
	12	7	ii. *t*.
	14	40	ii. T.
15	1	30	i. *o*.
	5	1	i. E.
	22	38	i. *t*.
16	0	52	i. T.
	6	18	ii. *o*.
	8	50	ii. O.
	9	3	ii. *e*.
	11	28	ii. E.
	19	59	i. *o*.
	22	51	iii. *o*.
	23	30	i. E.
17	1	6	iii. O.
	4	38	iii. *e*.
	6	11	iii. E.
	17	7	i. *t*.
	19	21	i. T.
18	1	27	ii. *t*.
	4	0	ii. T.
	14	28	i. *o*.
	17	59	i. E.
19	11	36	i. *t*.
	13	50	i. T.
	19	37	ii. *o*.
	22	10	ii. O.
	22	21	ii. *e*.
20	0	46	ii. E.
	8	57	i. *o*.
	12	28	i. E.
	12	57	iii. *t*.
	15	13	iii. T.
21	6	6	i. *t*.
	8	19	i. T.
	14	47	ii. *t*.
	17	20	ii. T.
22	3	27	i. *o*.
	6	57	i. E.
23	0	35	i. *t*.
	2	49	i. T.
	8	57	ii. *o*.
	11	30	ii. O.
	11	40	ii. *c*.

Jan. (con.).

d	h	m	
23	14	5	ii. E.
	21	56	i. *o*.
24	1	26	i. E.
	2	57	iii. *o*.
	5	13	iii. O.
	8	41	iii. *e*.
	10	12	iii. E.
	19	4	i. *t*.
	21	18	i. T.
25	4	8	ii. *t*.
	6	41	ii. T.
	16	25	i. *o*.
	19	55	i. E.
26	13	33	i. *t*.
	15	47	i. T.
	22	18	ii. *o*.
27	0	50	ii. O.
	0	59	ii. *e*.
	3	24	ii. E.
	10	55	i. *o*.
	14	24	i. E.
	17	6	iii. *t*.
	19	22	iii. T.
28	8	3	i. *t*.
	10	17	i. T.
	17	29	ii. *t*.
	20	2	ii. T.
29	5	24	i. *o*.
	8	53	i. E.
30	2	33	i. *t*.
	4	46	i. T.
	11	38	ii. *o*.
	14	11	ii. O.
	14	18	ii. *e*.
	16	43	ii. E.
	23	54	i. *o*.
31	3	22	i. E.
	7	8	iii. *o*.
	9	24	iii. O.
	12	43	iii. *e*.
	14	14	iii. E.
	21	2	i. *t*.
	23	16	i. T.

February.

d	h	m	
1	6	50	ii. *t*.
	9	23	ii. T.
	18	23	i. *o*.
	21	51	i. E.
2	15	32	i. *t*.
	17	46	i. T.
3	1	0	ii. *o*.
	3	33	ii. O.

Feb. (con.).

d	h	m	
3	3	37	ii. *e*.
	6	1	ii. E.
	12	53	i. *o*.
	16	19	i. E.
	21	19	iii. *t*.
	23	35	iii. T.
4	10	1	i. *t*.
	12	15	i. T.
	20	12	ii. *t*.
	22	45	ii. T.
5	7	23	i. *o*.
	10	48	i. E.
6	4	31	i. *t*.
	6	45	i. T.
	14	21	ii. *o*.
	16	54	ii. O.
	16	56	ii. *e*.
	19	21	ii. E.
7	1	52	i. *o*.
	5	17	i. E.
	11	22	iii. *o*.
	13	39	iii. O.
	16	46	iii. *e*.
	18	16	iii. E.
	23	1	i. *t*.
8	1	15	i. T.
	9	35	ii. *t*.
	12	7	ii. T.
	20	22	i. *o*.
	23	46	i. E.
9	17	31	i. *t*.
	19	45	i. T.
10	3	44	ii. *o*.
	8	39	ii. E.
	14	52	i. *o*.
	18	15	i. E.
11	1	34	iii. *t*.
	3	50	iii. T.
	12	1	i. *t*.
	14	14	i. T.
	22	56	ii. *t*.
12	1	30	ii. T.
	9	22	i. *o*.
	12	44	i. E.
13	6	31	i. *t*.
	8	44	i. T.
	17	6	ii. *o*.
	21	58	ii. E.
14	3	52	i. *o*.
	7	13	i. E.
	15	40	iii. *o*.
	17	56	iii. O.
	20	49	iii. *e*.
	22	18	iii. E.

Feb. (con.).

d	h	m	
15	1	0	i. *t*.
	3	14	i. T.
	12	19	ii. *t*.
	14	53	ii. T.
	22	21	i. *o*.
16	1	42	i. E.
	19	30	i. *t*.
	21	44	i. T.
17	6	29	ii. *o*.
	11	17	ii. E.
	16	51	i. *o*.
	20	11	i. E.
18	5	53	iii. *t*.
	8	9	iii. T.
	14	0	i. *t*.
	16	14	i. T.
19	1	42	ii. *t*.
	4	16	ii. T.
	11	21	i. *o*.
	14	39	i. E.
20	8	30	i. *t*.
	10	44	i. T.
	19	53	ii. *o*.
21	0	36	ii. E.
	5	51	i. *o*.
	9	8	i. E.
	20	1	iii. *o*.
	22	16	iii O.
22	0	51	iii. *e*.
	2	19	iii. E.
	3	1	i. *t*.
	5	14	i. T.
	15	6	ii. *t*.
	17	39	ii. T.
23	0	22	i. *o*.
	3	37	i. E.
	21	31	i. *t*.
	23	44	i. T.
24	9	17	ii. *o*.
	13	55	ii. E.
	18	52	i. *o*.
	22	6	i. E.
25	10	15	iii. *t*.
	12	30	iii. T.
	16	1	i. *t*.
	18	15	i. T.
26	4	29	ii. *t*.
	7	2	ii. T.
	13	22	i. *o*.
	16	35	i. E.
27	10	31	i. *t*.
	12	45	i. T.
	22	41	ii. *o*.
28	3	14	ii. E.

Feb. (con.).

d	h	m	
28	7	52	i. *o*.
	11	4	i. E.

March.

d	h	m	
1	0	24	iii. *o*.
	2	38	iii. O.
	4	54	iii. *e*.
	5	1	i. *t*.
	6	21	iii. E.
	7	15	i. T.
	17	53	ii. *t*.
	20	26	ii. T.
2	2	22	i. *o*.
	5	33	i. E.
	23	31	i. *t*.
3	1	45	i. T.
	12	5	ii. *o*.
	16	33	ii. E.
	20	52	i. *o*.
4	0	1	i. E.
	14	40	iii. *t*.
	16	53	iii. T.
	18	2	i. *t*.
	20	16	i. T.
5	7	17	ii. *t*.
	9	50	ii. T.
	15	23	i. *o*.
	18	30	i. E.
6	12	32	i. *t*.
	14	46	i. T.
7	1	30	ii. *o*.
	5	52	ii. E.
	9	53	i. *o*.
	12	59	i. E.
8	4	49	iii. *o*.
	7	2	iii. O.
	7	2	i. *t*.
	8	55	iii. *e*.
	9	16	i. T.
	10	22	iii. E.
	20	41	ii. *t*.
	23	14	ii. T.
9	4	23	i. *o*.
	7	28	i. E.
10	1	33	i. *t*.
	3	46	i. T.
	14	54	ii. *o*.
	19	11	ii. E.
	22	53	i. *o*.
11	1	57	i. E.
	19	7	iii. *t*.
	20	3	i. *t*.
	21	19	iii. T.

March (con.).

d	h	m	
11	22	17	i. T.
12	10	5	ii. *t*.
	12	38	ii. T.
	17	24	i. *o*.
	20	25	i. E.
13	14	34	i. *t*.
	16	47	i. T.
14	4	19	ii. *o*.
	8	31	ii. E.
	11	54	i. *o*.
	14	54	i. E.
15	9	4	i. *t*.
	9	16	iii. *o*.
	11	18	i. T.
	11	27	iii. O.
	12	57	iii. *e*.
	14	23	iii. E.
	23	29	ii. *t*.
16	2	3	ii. T.
	6	24	i. *o*.
	9	23	i. E.
17	3	34	i. *t*.
	5	48	i. T.
	17	45	ii. *o*.
	21	49	ii. E.
18	0	55	i. *o*.
	3	52	i. E.
	22	5	i. *t*.
	23	35	iii. *t*.
19	0	19	i. T.
	1	45	iii. T.
	12	54	ii. *t*.
	15	27	ii. T.
	19	25	i. *o*.
	22	20	i. E.
20	16	35	i. *t*.
	18	49	i. T.
21	7	10	ii. *o*.
	11	9	ii. E.
	13	55	i. *o*.
	16	49	i. E.
22	11	6	i. *t*.
	13	20	i. T.
	13	45	iii. *o*.
	15	54	iii. O.
	16	59	iii. *e*.
	18	24	iii. E.
23	2	18	ii. *t*.
	4	52	ii. T.
	8	26	i. *o*.
	11	18	i. E.
24	5	36	i. *t*.
	7	50	i. T.
	20	36	ii. *o*.
25	0	27	ii. E.
	2	56	i. *o*.
	5	47	i. E.
26	0	7	i. *t*.
	2	21	i. T.
	4	4	iii. *t*.
	6	13	iii. T.
	15	43	ii. *t*.
	18	17	ii. T.
	21	27	i. *o*.
27	0	15	i. E.
	18	37	i. *t*.
	20	51	i. T.
28	10	2	ii. *o*.
	13	47	ii. E.
	15	57	i. *o*.
	18	44	i. E.
29	13	8	i. *t*.
	15	22	i. T.
	18	14	iii. *o*.
	20	22	iii. O.
	21	1	iii. *e*.
	22	25	iii. E.
30	5	8	ii. *t*.
	7	41	ii. T.
	10	28	i. *o*.
	13	13	i. E.
31	7	39	i. *t*.
	9	52	i. T.
	23	28	ii. *o*.

April.

d	h	m	
1	3	6	ii. E.
	4	58	i. *o*.
	7	42	i. E.
2	2	9	i. *t*.
	4	23	i. T.
	8	35	iii. *t*.
	10	42	iii. T.
	18	32	ii. *t*.
	21	6	ii. T.
	23	29	i. *o*.
3	2	10	i. E.
	20	40	i. *t*.
	22	53	i. T.
4	12	54	ii. *o*.
	16	25	ii. E.
	17	59	i. *o*.
	20	39	i. E.
5	15	11	i. *t*.
	17	24	i. T.
	22	46	iii. *o*.
6	0	52	iii. O.
	1	3	iii. *e*.
	2	27	iii. E.
	7	57	ii. *t*.
	10	31	ii. T.
	12	30	i. *o*.
	15	8	i. E.

June.

d	h	m	
1	7	30	i. *t*.
	9	43	i. T.
2	4	17	i. *e*.
	6	34	ii. *t*.
	6	57	i. O.
	9	7	ii. T.
	9	12	iii. *e*.
	10	33	iii. E.
	11	1	iii. *o*.
	12	51	iii. O.
3	2	1	i. *t*.
3	4	13	i. T.
	22	45	i. *e*.
4	0	22	ii. *e*.
	1	27	i. O.
	3	58	ii. O.
	20	31	i. *t*.
	22	44	i. T.
5	17	14	i. *e*.
	19	57	i. O.
	19	58	ii. *t*.
	22	31	ii. T.
6	1	24	iii. *t*.
	3	13	iii. T.
	15	2	i. *t*.
	17	14	i. T.
7	11	42	i. *e*.
	13	41	ii. *e*.
	14	27	i. O.
	17	23	ii. O.
8	9	32	i. *t*.
	11	45	i. T.
9	6	11	i. *e*.
	8	58	i. O.
	9	22	ii. *t*.
	11	55	ii. T.
	13	12	iii. *e*.
	14	34	iii. E.
	15	30	iii. *o*.
	17	19	iii. O.
10	4	2	i. *t*.
	6	15	i. T.
11	0	39	i. *e*.
	2	59	ii. *e*.
	3	28	i. O.
	6	49	ii. O.
	22	33	i. *t*.
12	0	45	i. T.
	19	8	i. *e*.
	21	58	i. O.
	22	46	i. *t*.
13	1	19	ii. T.
	5	53	iii. *t*.
	7	41	iii. T.
	17	3	i. *t*.
	19	16	i. T.
14	13	36	i. *e*.
	16	18	ii. *e*.
	16	28	i. O.
	20	15	ii. O.
15	11	33	i. *t*.
	13	46	i. T.
16	8	5	i. *e*.
	10	58	i. O.
	12	10	ii. *t*.
	14	43	ii. T.
	17	12	iii. *e*.
	18	34	iii E.
	19	58	iii. *o*.
	21	45	iii. O.
17	6	3	i. *t*.
	8	16	i. T.
18	2	33	i. *e*.
	5	28	i. O.
	5	37	ii. *e*.
18	9	40	ii. O.
19	0	34	i. *t*.
	2	46	i. T.
	21	2	i. *e*.
	23	58	i. O.
20	1	33	ii. *t*.
	4	6	ii. T.
	10	22	iii. *t*.
	12	8	iii. T.
	19	4	i. *t*.
	21	16	i. T.
21	15	30	i. *e*.
	18	28	i. O.
	18	56	ii. *e*.
	23	5	ii. O.
22	13	34	i. *t*.
	15	47	i. T.
23	9	59	i. *e*.
	12	58	i. O.
	14	57	ii. *t*.
	17	29	ii. T.
	21	12	iii. *e*.
	22	34	iii. E.
24	0	24	iii. *o*.
	2	10	iii. O.
	8	4	i. *t*.
	10	17	i. T.
25	4	27	i. *e*.
	7	28	i. O.
	8	14	ii. *e*.
	12	30	ii. O.
26	2	34	i. *t*.
	4	47	i. T.
	22	56	i. *e*.
27	1	58	i. O.
	4	20	ii. *t*.
	6	53	ii. T.
	14	48	iii. *t*.
	16	33	iii. T.
	21	5	i. *t*.
	23	17	i. T.
28	17	24	i. *e*.
	20	28	i. O.
	21	33	ii. *e*.
29	1	54	ii. O.
	15	35	i. *t*.
	17	47	i. T.
30	11	53	i. *e*.
	14	58	i. O.
	17	43	ii. *t*.
	20	16	ii. T.

July.

d	h	m	
1	1	13	iii. *e*.
	2	34	iii. E.
	4	49	iii. *o*.
	6	34	iii. O.
	10	5	i. *t*.
	12	17	i. T.
2	6	21	i. *e*.
	9	28	i. O.
	10	51	ii. *e*.
	15	18	ii. O.
3	4	35	i. *t*.
	6	47	i. T.
4	0	50	i. *e*.
	3	58	i. O.
	7	5	ii. *t*.
	9	38	ii. T.
	19	12	iii. *t*.
	20	56	iii. T.
	23	5	i. *t*.
5	1	17	i. T.
	19	18	i. *e*.
	22	28	i. O.
6	0	10	ii. *e*.
	4	42	ii. O.
	17	35	i. *t*.
	19	47	i. T.
7	13	47	i. *e*.
	16	58	i O.
	20	29	ii. *t*.
	23	2	ii. T.
8	5	13	iii. *e*.
	6	35	iii. E.
	9	13	iii. *o*.
	10	56	iii. O.
	12	5	i. *t*.
	14	17	i. T
9	8	15	i. *e*.
	11	27	i. O.
	13	28	ii. *e*.
	18	6	ii. O.
10	6	34	i. *t*.
	8	47	i. T.
11	2	43	i. *e*.
	5	57	i O.
	9	51	ii. *t*.
	12	24	ii. T.
	23	35	iii. *t*.
12	1	4	i. *t*.
	1	17	iii. T.
	3	16	i. T.
	21	12	i. *e*.
13	0	27	i. O.
	2	47	ii. *e*.
	7	30	ii. O.
	19	34	i. *t*.
	21	46	i. T.
14	15	40	i. *e*.
	18	56	i. O.
	23	14	ii. *t*.
15	1	47	ii. T.
	9	14	iii. *e*.
	10	36	iii. E.
	13	35	iii. *o*.
	14	4	i. *t*.
	15	16	iii. O.
	16	16	i. T.
16	10	9	i. *e*.
	13	26	i. O.
	16	5	ii. *e*.
	20	53	ii. O.
17	8	33	i. *t*.
	10	46	i. T.
18	4	37	i. *e*.
	7	55	i. O.
18	12	36	ii. *t*.
	15	9	ii. T.
19	3	3	i. *t*.
	3	55	iii. *t*.
	5	15	i. T.
	5	35	iii. T.
	23	6	i. *e*.
20	2	25	i. O.
	5	23	ii. *e*.
	10	16	ii. O.
	21	33	i. *t*.
	23	45	i. T.
21	17	34	i. *e*.
	20	54	i. O.
22	1	57	ii. *t*.
	4	30	ii. T.
	13	14	iii. *e*.
	14	36	iii. E.
	16	2	i. *t*.
	17	54	iii. *o*.
	18	14	i. T.
	19	33	iii. O.
23	12	3	i. *e*.
	15	24	i. O.
	18	41	ii. *e*.
	23	38	ii. O.
24	10	32	i. *t*.
	12	44	i. T.
25	6	31	i. *e*.
	9	53	i. O.
	15	19	ii. *t*.
	17	52	ii. T.
26	5	1	i. *t*.
	7	13	i. T.
	8	13	iii. *t*.
	9	52	iii. T.
27	0	59	i. *e*.
	4	23	i. O.
	8	0	ii. *e*.
	13	0	ii. O.
	23	31	i. *t*.
28	1	43	i. T.
	19	28	i. *e*.
	22	52	i. O.
29	4	40	ii. *t*.
	7	13	ii. T.
	17	14	iii. *e*.
	18	0	i. *t*.
	18	36	iii. E.
	20	12	i. T.
	22	10	iii. *o*.
	23	48	iii. O.
30	13	56	i. *e*.
	17	21	i. O.
	21	18	ii. *e*.
	23	45	ii. E.
	23	49	ii. *o*.
31	2	22	ii. O.
	12	30	i. *t*.
	14	42	i. T.

August.

d	h	m	
1	8	25	i. *e*.

Aug. (con.).

d	h	m	
1	11	51	i. O.
	18	1	ii. *t*.
	20	34	ii. T.
2	6	59	i. *t*.
	9	11	i. T.
	12	28	iii. *t*.
	14	6	iii. T.
3	2	53	i. *e*.
	6	20	i. O.
	10	36	ii. *e*.
	13	4	ii. E.
	13	10	ii. *o*.
	15	43	ii. O.
4	1	28	i. *t*.
	3	40	i. T.
	21	22	i. *e*.
5	0	49	i. O.
	7	21	ii. *t*.
	9	54	ii. T.
	19	58	i. *t*.
	21	13	iii. *e*.
	22	10	i. T.
	22	36	iii. E.
6	2	22	iii. *o*.
	4	0	iii. O.
	15	50	i. *e*.
	19	18	i. O.
	23	54	ii. *e*.
7	2	22	ii. E.
	2	30	ii. *o*.
	5	3	ii. O.
	14	27	i. *t*.
	16	39	i. T.
8	10	19	i. *e*.
	13	47	i. O.
	20	41	ii. *t*.
	23	14	ii. T.
9	8	56	i. *t*.
	11	8	i. T.
	16	40	iii. *t*.
	18	17	iii. T.
10	4	47	i. *e*.
	8	16	i. O.
	13	12	ii. *e*.
	15	40	ii. E.
	15	51	ii. *o*.
	18	24	ii. O.
11	3	25	i. *t*.
	5	37	i. T.
	23	15	i. *e*.
12	2	45	i. O.
	10	1	ii. *t*.
	12	34	ii. T.
	21	54	i. *t*.
13	0	6	i. T.
	1	13	iii. *e*.
	2	36	iii. E.
	6	32	iii. *o*.
	8	8	iii. O.
	17	44	i. *e*.
	21	14	i. O.
14	2	30	ii. *e*.
	4	58	ii. E.
	5	11	ii. *o*.

Aug. (con.).

d	h	m	
14	7	43	ii. O.
	16	23	i. *t*.
	18	35	i. T.
15	12	12	i. *e*.
	15	43	i. O.
	23	20	ii. *t*.
16	1	53	ii. T.
	10	51	i. *t*.
	13	3	i. T.
	20	49	iii. *t*.
	22	24	iii. T.
17	6	41	i. *e*.
	10	12	i. O.
	15	48	ii. *e*.
	18	16	ii. E.
	18	30	ii. *o*.
	21	3	ii. O.
18	5	20	i. *t*.
	7	32	i. T.
19	1	9	i. *e*.
	4	40	i. O.
	12	38	ii. *t*.
	15	11	ii. T.
	23	49	i. *t*.
20	2	1	i. T.
	5	13	iii. *e*.
	6	37	iii. E.
	10	39	iii. *o*.
	12	13	iii. O.
	19	38	i. *e*.
	23	9	i. O.
21	5	6	ii. *e*.
	7	34	ii. E.
	7	49	ii. *o*.
	10	22	ii. O.
	18	18	i. *t*.
	20	30	i. T.
22	14	6	i. *e*.
	17	38	i. O.
23	1	58	i. *t*.
	4	31	ii. T.
	12	47	i. *t*.
	14	59	i. T.
24	0	54	iii. *t*.
	2	27	iii. T.
	8	35	i. *e*.
	12	6	i. O.
	18	24	ii. *e*.
	20	52	ii. E.
	21	7	ii. *o*.
	23	40	ii. O.
25	7	15	i. *t*.
	9	27	i. T.
26	3	3	i. *e*.
	6	35	i. O.
	15	16	ii. *t*.
	17	49	ii. T.
27	1	44	i. *t*.
	3	56	i. T.
	9	12	iii. *e*.
	10	37	iii. E.
	14	42	iii. *o*.
	16	14	iii. O.
	21	31	i. *e*.

Aug. (con.).

d	h	m	
28	1	4	i. O.
	7	41	ii. *e*.
	10	9	ii. E.
	10	25	ii. *o*.
	12	58	ii. O.
	20	12	i. *t*.
	22	24	i. T.
29	16	0	i. *e*.
	19	32	i. O.
30	4	34	ii. *t*.
	7	7	ii. T.
	14	41	i. *t*.
	16	52	i. T.
31	4	54	iii. *t*.
	6	25	iii. T.
	10	28	i. *e*.
	14	0	i. O.
	20	59	ii. *e*.
	23	28	ii. E.
	23	42	ii. *o*.

September.

d	h	m	
1	2	15	ii. O.
	9	9	i. *t*.
	11	21	i. T.
2	4	57	i. *e*.
	8	29	i. O.
	17	51	ii. *t*.
	20	24	ii. T.
3	3	37	i. *t*.
	5	49	i. T.
	13	13	iii. *e*.
	14	38	iii. E.
	18	41	iii. *o*.
	20	12	iii. O.
	23	25	i. *e*.
4	2	57	i. O.
	10	17	ii. *e*.
	12	45	ii. E.
	12	59	ii. *o*.
	15	32	ii. O.
	22	5	i. *t*.
5	0	17	i. T.
	17	54	i. *e*.
	21	25	i. O.
6	7	8	ii. *t*.
	9	41	ii. T.
	16	34	i. *t*.
	18	45	i. T.
7	8	50	iii. *t*.
	10	20	iii. T.
	12	22	i. *e*.
	15	53	i. O.
	23	35	ii. *e*.
8	2	3	ii. E.
	2	15	ii. *o*.
	4	48	ii. O.
	11	2	i. *t*.
	13	13	i. T.
9	6	51	i. *e*.
	10	21	i. O.
	20	24	ii. *t*.
	22	57	ii. T.

Sept. (con.).

d	h	m	
10	5	30	i. *t*.
	7	41	i. T.
	17	12	iii. *e*.
	18	38	iii. E.
	22	36	iii. *o*.
11	0	4	iii. O.
	1	19	i. *e*.
	4	49	i. O.
	12	52	ii. *e*.
	15	21	ii. E.
	15	31	ii. *o*.
	18	3	ii. O.
	23	58	i. *t*.
12	2	9	i. T.
	19	47	i. *e*.
	23	17	i. O.
13	9	40	ii. *t*.
	12	13	ii. T.
	18	25	i. *t*.
	20	37	i. T.
14	12	42	iii. *t*.
	14	11	iii. T.
	14	16	i. *e*.
	17	45	i. O.
15	2	10	ii. *e*.
	4	38	ii. E.
	4	45	ii. *o*.
	7	18	ii. O.
	12	53	i. *t*.
	15	5	i. T.
16	8	44	i. *e*.
	12	12	i. O.
	22	55	ii. *t*.
17	1	28	ii. T.
	7	21	i. *t*.
	9	33	i. T.
	21	12	iii. *e*.
	22	39	iii. E.
18	2	25	iii. *o*.
	3	13	i. *e*.
	3	52	iii. O.
	6	40	i. O.
	15	27	ii. *e*.
	17	56	ii. E.
	18	0	ii. *o*.
	20	32	ii. O.
19	1	48	i. *t*.
	4	0	i. T.
	21	41	i. *e*.
20	1	8	i. O.
	12	9	ii. *t*.
	14	41	ii. T.
	20	16	i. *t*.
	22	28	i. T.
21	16	10	i. *e*.
	16	30	iii. *t*.
	17	57	iii. T.
	19	35	i. O.
22	4	45	ii. *e*.
	9	46	ii. O.
	14	44	i. *t*.
	16	55	i. T.
23	10	38	i. *e*.
	14	3	i. O.

Sept. (con.).

d	h	m	
24	1	24	ii. *t*.
	3	56	ii. T.
	9	11	i. *t*.
	11	23	i. T.
25	1	11	iii. *e*.
	2	39	iii. E.
	5	7	i. *e*.
	6	10	iii. *o*.
	7	36	iii. O.
	8	30	i. O.
	18	2	ii. *e*.
	22	59	ii. O.
26	3	38	i. *t*.
	5	50	i. T.
	23	35	i. *e*.
27	2	57	i. O.
	14	38	ii. *t*.
	17	10	ii. T.
	22	6	i. *t*.
28	0	17	i. T.
	18	4	i. *e*.
	20	13	iii. *t*.
	21	25	i. O.
	21	38	iii. T.
29	7	20	ii. *e*.
	12	12	ii. O.
	16	33	i. *t*.
	18	44	i. T.
30	12	32	i. *e*.
	15	52	i. O.

October.

d	h	m	
1	3	50	ii. *t*.
	6	22	ii. T.
	11	0	i. *t*.
	13	12	i. T.
2	5	11	iii. *e*.
	6	39	iii. E.
	7	1	i. *e*.
	9	50	iii. *o*.
	10	19	i. O.
	11	14	iii. O.
	20	37	ii. *e*.
3	1	23	ii. O.
	5	27	i. *t*.
	7	39	i. T.
4	1	29	i. *e*.
	4	46	i. O.
	17	3	ii. *t*.
	19	35	ii. T.
	23	54	i. *t*.
5	2	5	i. T.
	19	58	i. *e*.
	23	13	i. O.
	23	51	iii. *t*.
6	1	14	iii. T.
	9	55	ii. *e*.
	14	35	ii. O.
	18	21	i. *t*.
	20	32	i. T.
7	14	27	i. *e*.
	17	40	i. O.
8	6	14	ii. *t*.

Oct. (con.).

d	h	m	
8	8	47	ii. T.
	12	48	i. *t*.
	14	59	i. T.
9	8	55	i. *e*.
	9	11	iii. *e*.
	10	40	iii. E.
	12	7	i. O.
	13	26	iii. *o*.
	14	48	iii. O.
	23	12	ii. *e*.
10	3	45	ii. O.
	7	14	i. *t*.
	9	26	i. T.
11	3	24	i. *e*.
	6	34	i. O.
	19	26	ii. *t*.
	21	58	ii. T.
12	1	41	i. *t*.
	3	53	i. T.
	21	52	i. *e*.
13	1	0	i. O.
	3	24	iii. *t*.
	4	46	iii. T.
	12	30	ii. *e*.
	16	55	ii. O.
	20	8	i. *t*.
	22	19	i. T.
14	16	21	i. *e*.
	19	27	i. O.
15	8	37	ii. *t*.
	11	9	ii. T.
	14	34	i. *t*.
	16	46	i. T.
16	10	49	i. *e*.
	13	11	iii. *e*.
	13	54	i. O.
	14	41	iii. E.
	16	56	iii. *o*.
	18	17	iii. O.
17	1	47	ii. *e*.
	6	5	ii. O.
	9	1	i. *t*.
	11	12	i. T.
18	5	18	i. *e*.
	8	20	i. O.
	21	46	ii. *t*.
19	0	18	ii. T.
	3	27	i. *t*.
	5	39	i. T.
	23	46	i. *e*.
20	2	47	i. O.
	6	51	iii. *t*.
	8	12	iii. T.
	15	4	ii. *e*.
	19	14	ii. O.
	21	54	i. *t*.
21	0	5	i. T.
	18	15	i. *e*.
	21	13	i. O.
22	10	57	ii. *t*.
	13	28	ii. T.
	16	20	i. *t*.
	18	31	i. T.
23	12	44	i. *e*.

Oct. (con.).

d	h	m	
23	15	40	i. O.
	17	11	iii. *e*.
	18	43	iii. E.
	20	23	iii. *o*.
	21	43	iii. O.
24	4	22	ii. *e*.
	8	22	ii. O.
	10	46	i. *t*.
	12	57	i. T.
25	7	12	i. *e*.
	10	6	i. O.
26	0	6	ii. *t*.
	2	38	ii. T.
	5	12	i. *t*.
	7	24	i. T.
27	1	41	i. *e*.
	4	32	i. O.
	10	15	iii. *t*.
	11	35	iii. T.
	17	39	ii. *e*.
	21	30	ii. O.
	23	39	i. *t*.
28	1	50	i. T.
	20	9	i. *e*.
	22	59	i. O.
29	13	15	ii. *t*.
	15	46	ii. T.
	18	5	i. *t*.
	20	16	i. T.
30	14	38	i. *e*.
	17	25	i. O.
	21	11	iii. *e*.
	22	44	iii. E.
	23	45	iii. *o*.
31	1	5	iii. O.
	6	57	ii. *e*.
	10	38	ii. O.
	12	31	i. *t*.
	14	42	i. T.

November.

1	9	7	i. *e*.
	11	51	i. O.
2	2	24	ii. *t*.
	4	56	ii. T.
	6	57	i. *t*.
	9	8	i. T.
3	3	35	i. *e*.

Nov. (con.).

d	h	m	
3	6	17	i. O.
	13	35	iii. *t*.
	14	56	iii. T.
	20	14	ii. *e*.
	23	45	ii. O.
4	1	23	i. *t*.
	3	34	i. T.
	22	4	i. *e*.
5	0	43	i. O.
	15	31	ii. *t*.
	18	3	ii. T.
	19	49	i. *t*.
	22	0	i. T.
6	16	33	i. *e*.
	19	9	i. O.
7	1	11	iii. *e*.
	2	45	iii. E.
	3	3	iii. *o*.
	4	24	iii. O.
	9	31	ii. *e*.
	12	52	ii. O.
	14	15	i. *t*.
	16	26	i. T.
8	11	1	i. *e*.
	13	35	i. O.
9	4	40	ii. *t*.
	7	11	ii. T.
	8	41	i. *t*.
	10	52	i. T.
10	5	30	i. *e*.
	8	1	i. O.
	16	52	iii. *t*.
	18	14	iii. T.
	22	49	ii. *e*.
11	1	59	ii. O.
	3	6	i. *t*.
	5	18	i. T.
	23	59	i. *e*.
12	2	27	i. O.
	17	47	ii. *t*.
	20	18	ii. T.
	21	32	i. *t*.
	23	43	i. T.
13	18	27	i. *e*.
	20	53	i. O.
14	5	11	iii. *e*.
	7	41	iii. O.
	12	6	ii. *e*.
	15	6	ii. O.

Nov. (con.).

d	h	m	
14	15	58	i. *t*.
	18	9	i. T.
15	12	56	i. *e*.
	15	19	i. O.
16	6	55	ii. *t*.
	9	26	ii. T.
	10	24	i. *t*.
	12	35	i. T.
17	7	25	i. *e*.
	9	45	i. O.
	20	7	iii. *t*.
	21	31	iii. T.
18	1	23	ii. *e*.
	4	12	ii. O.
	4	50	i. *t*.
	7	1	i. T.
19	1	53	i. *e*.
	4	11	i. O.
	20	2	ii. *t*.
	22	34	ii. T.
	23	15	i. *t*.
20	1	27	i. T.
	20	22	i. *e*.
	22	37	i. O.
21	9	11	iii. *e*.
	10	57	iii. O.
	14	41	ii. *e*.
	17	18	ii. O.
	17	41	i. *t*.
	19	52	i. T.
22	14	51	i. *e*.
	17	3	i. O.
23	9	10	ii. *t*.
	11	42	ii. T.
	12	7	i. *t*.
	14	18	i. T.
24	9	17	i. *o*.
	11	29	i. O.
	23	21	iii. *t*.
25	0	47	iii. T.
	3	53	ii. *o*.
	6	28	ii. E.
	6	32	i. *t*.
	8	44	i. T.
26	3	43	i. *o*.
	5	57	i. E.
	22	17	ii. *t*.
27	0	49	ii. T.
	0	58	i. *t*.

Nov. (con.).

d	h	m	
27	3	10	i. T.
	22	9	i. *o*.
28	0	26	i. E.
	12	46	iii. *o*.
	14	49	iii. E.
	17	0	ii. *o*.
	19	24	i. *t*.
	19	46	ii. E.
	21	36	i. T.
29	16	35	i. *o*.
	18	54	i. E.
30	11	25	ii. *t*.
	13	50	i. *t*.
	13	57	ii. T.
	16	2	i. T.

December.

1	11	1	i. *o*.
	13	23	i. E.
2	2	34	iii. *t*.
	4	3	iii. T.
	6	6	ii. *o*.
	8	16	i. *t*.
	9	3	ii. E.
	10	27	i. T.
3	5	27	i. *o*.
	7	52	i. E.
4	0	32	ii. *t*.
	2	42	i. *t*.
	3	4	ii. T.
	4	53	i. T.
	23	53	i. *o*.
5	2	21	i. E.
	16	0	iii. *o*.
	18	50	iii. E.
	19	13	ii. *o*.
	21	7	i. *t*.
	22	21	ii. E.
	23	19	i. T.
6	18	19	i. *o*.
	20	50	i. E.
7	13	40	ii. *t*.
	15	33	i. *t*.
	16	12	ii. T.
	17	45	i. T.
8	12	45	i. *o*.
	15	19	i. E.

Dec. (con.).

d	h	m	
9	5	48	iii. *t*.
	7	21	iii. T.
	8	19	ii. *o*.
	9	59	i. *t*.
	11	39	ii. E.
	12	11	i. T.
10	7	11	i. *o*.
	9	47	i. E.
11	2	48	ii. *t*.
	4	25	i. *t*.
	5	20	ii. T.
	6	37	i. T.
12	1	38	i. *o*.
	4	16	i. E.
	19	17	iii. *o*.
	20	51	iii. O.
	21	14	iii. *e*.
	21	27	ii. *o*.
	22	52	i. *t*.
	22	53	iii. E.
13	6	56	ii. E.
	1	3	i. T.
	20	4	i. *o*.
	22	45	i. E.
14	15	57	ii. *t*.
	17	18	i. *t*.
	18	29	ii. T.
	19	30	i. T.
15	14	30	i. *o*.
	17	14	i. E.
16	9	5	iii. *t*.
	10	34	ii. *o*.
	10	41	iii. T.
	11	44	i. *t*.
	13	56	i. T.
	14	14	ii. E.
17	8	57	i. *o*.
	11	43	i. E.
18	5	5	ii. *t*.
	6	10	i. *t*.
	7	38	ii. T.
	8	22	i. T.
19	3	23	i. *o*.
	6	12	i. E.
	22	36	iii. *o*.
	23	42	ii. *o*.
20	0	14	iii. O.
	0	36	i. *t*.
	1	14	iii. *e*.

Dec. (con.).

d	h	m	
20	2	48	i. T.
	2	55	iii. E.
	3	32	ii. E.
	21	49	i. *o*.
21	0	40	i. E.
	18	15	ii. *t*.
	19	3	i. *t*.
	20	48	ii. T.
	21	15	i. T.
22	16	16	i. *o*.
	19	9	i. E.
23	12	26	iii. *t*.
	12	50	ii. *o*.
	13	29	i. *t*.
	14	6	iii. T.
	15	41	i. T.
	16	49	ii. E.
24	10	43	i. *o*.
	13	38	i. E.
25	7	24	ii. *t*.
	7	56	i. *t*.
	9	58	ii. T.
	10	8	i. T.
26	5	9	i. *o*.
	8	7	i. E.
27	1	58	iii. *o*.
	1	59	ii. *o*.
	2	22	i. *t*.
	3	40	iii. O.
	4	34	i. T.
	5	15	iii. *e*.
	6	7	ii. E.
	6	57	iii. E.
	23	36	i. *o*.
28	2	36	i. E.
	20	35	ii. *t*.
	20	49	i. *t*.
	23	1	i. T.
	23	9	ii. T.
29	18	3	i. *o*.
	21	5	i. E.
30	15	9	ii. *o*.
	15	15	i. *t*.
	15	51	iii. *t*.
	17	28	i. T.
	17	34	iii. T.
	19	25	ii. E.
31	12	30	i. *o*.
	15	34	i. E.

The Satellites of Jupiter will be invisible from April 7 until June 1, Jupiter being too near the Sun.

Satellites I. & II. *reappear* at eclipse on the *f* side in January, February, March, April, and December, and *disappear* on the *p* side from June to November. Satellite II. *reappears* on the *p* side in August and September.

Satellite III. *disappears* and *reappears* on the *f* side in January, February, March, April, and December, and on the *p* side from June to November.

There will be no eclipse of Satellite IV. throughout the year.

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Jan. 8h	Feb. 8h	March. 7½h	April. 7½h	June. 15h	July. 14h	Aug. 14h	Sept. 13h	Oct. 12½h	Nov. 12h	Dec. 11h	Day.
1	32O14	♃41O3	41O32	O243●	31O24	421O3	4O123	341O2	♃32O4	O1234	214O3	1
2	13O24	24O13	42O13	1O432	2O314	4O13●	♃421O	43O21	2O314	21O34	31O24	2
3	O1234	1O234	412O3	342O1	21O34	14O23	43O1●	4213O	1O243	2O314	3O124	3
4	21O34	3O124	4O312	4312O	O1234	2O341	431O2	4O13●	4O213	31O24	321O4	4
5	♃2O34	32O4●	♃431O	43O12	1O234	321O4	432O1	41O23	421O3	3O214	23O14	5
6	3O24●	321O4	432O1	41O23	23O14	3O124	214O3	42O13	43O1●	231O4	1O234	6
7	31O24	3O124	341O2		3O14●	3O24●	O1243	423O●	431O2	4O13●	O2134	7
8	324O1	1O234	♃O342		31O42	♃2O34	O1234	♃34O2	432O1	4O23●	21O34	8
9	431O●	2O143	2O143		423O1	O134●	21O34	3O412	423O1	421O3	♃3O4●	9
10	4O132	1O43●	21O34		421O3	1O234	3O14●	231O4	41O23	42O31	34O12	10
11	421O3	43O12	O1324		4O123	2O314	31O24	O134●	4O123	431O2	4321O	11
12	42O13	4321O	13O24		41O23	2314O	32O14	1O234	214O3	43O21	423O1	12
13	4O32●	♃432O	32O14		423O1	34O12	21O34	2O134	3O14●	4321O	41O23	13
14	431O2	43O12	31O4●		432O●	431O2	O1423	♃21O4	31O24	42O13	4O213	14
15	342O1	41O23	3O124		431O2	42O1●	4O23●	♃3O24	32O14	1O423	421O3	15
16	314O●	42O13	2O43●		432O1	42O13	421O3	3O124	23O4●	♃2O34	43O1●	16
17	O3142	41O3●	421O3		21O43	41O23	432O1	231O4	1O234	2O134	34O2●	17
18	12O34	♃4O12	4O123		O2143	♃4O13	431O2	42O13	O1234	31O24	321O4	18
19	2O134	321O4	413O2		1O234	4213O	♃43O1	41O23	21O34	3O214	23O14	19
20	1O324	32O14	432O1		23O14	34O21	421O3	♃4O13	23O41	321O4	1O234	20
21	♃3O24	3O24●	431O●		321O4	31O42	4O213	421O3	341O2	2O314	O2134	21
22	32O14	1O234	43O12		♃3O24	23O14	41O23	43O12	♃43O1	1O423	21O34	22
23	312O4	2O134	421O3		♃3O14	2O34●	♃2O43	43O2●	4231O	42O13	2O314	23
24	O3142	12O34	♃24O3		21O34	1O234	32O14	4321O	♃4O23	42O13	3O24●	24
25	142O3	O3124	O1423		O4213	O2134	31O24	24O31	4O123	431O2	321O4	25
26	42O13	312O4	13O24		41O23	213O4	3O214	1O423	421O3	43O12	234O1	26
27	41O32	32O14	32O14		♃42O1	3O214	213O4	O2134	423O1	4321O	41O32	27
28	43O12	34O2●	312O4		4321O	31O42	O2134	21O34	341O2	423O1	4O213	28
29	432O●		3O124		43O12	234O1	1O234	3O214	3O241	41O23	421O3	29
30	4312O		♃1O34		43O12	42O3●	2O134	3O24●	231O4	♃4O13	42O31	30
31	4O12●		2O134			♃4O23	23O4●		O1234		431O2	31

The circle (O) represents Jupiter; ♃ signifies that the satellite is on the disk; ● signifies that the satellite is behind the disk or in the shadow.

EPHEMERIS OF JUPITER'S SATELLITE V.

Greenwich Mean Time of every twentieth Eastern Elongation. The times of other Elongations can be found by adding multiples of 11h·96 to these.

	d	h		d	h		d	h		d	h
Jan.	1	10·5	July	26	5·9	Sept.	24	0·7	Nov.	22	19·2
	11	9·7	Aug.	5	5·0	Oct.	3	23·8	Dec.	2	18·3
	21	8·9		15	4·2		13	22·9		12	17·4
	31	8·0		25	3·3		23	22·0		22	16·5
Feb.	10	7·3	Sept.	4	2·5	Nov.	2	21·1			
	20	6·5		14	1·6		12	20·2			

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.		Z. D.	Mean Refraction.		Z. D.	Mean Refraction.		Z. D.	Mean Refraction.	
°	′	″	°	′	″	°	′	″	°	′	″
0		0·0	56	1	25·4	68	2	21·9	78	4	25·0
10		10·2	58	1	32·1	69	2	29·3	79	4	48·5
20		21·0	60	1	39·7	70	2	37·3	80	5	16·2
30		33·3	61	1	43·8	71	2	46·1	81	5	49·3
35		40·4	62	1	48·2	72	2	55·8	82	6	29·6
40		48·4	63	1	52·8	73	3	6·6	83	7	19·7
45		57·7	64	1	57·8	74	3	18·6	84	8	23·3
50	1	8·7	65	2	3·2	75	3	32·1	85	9	46·5
52	1	13·8	66	2	8·9	76	3	47·4			
54	1	19·3	67	2	15·2	77	4	4·9			

SATELLITES OF MARS.

East Elongations.

Phobos.			Deimos.		
April.	May.	June.	April.	May.	June.
d h	d h	d h	d h	d h	d h
19 17·9	3 3·3	1 3·6	18 5·2	3 8·4	2 14·9
21 23·4	5 8·8	3 9·1	22 0·0	7 3·2	6 9·7
24 5·0	7 14·4	5 14·7	25 18·8	10 22·0	10 4·7
26 10·6	9 20·0	7 20·3	29 13·6	14 16·8	
28 16·1	12 1·5			18 11·6	
30 21·7	14 7·1			22 6·4	
	16 12·6			26 1·2	
	18 18·2			29 20·1	
	20 23·7				
	23 5·3				
	25 10·9				
	27 16·4				
	29 22·0				

For Phobos every seventh elongation is given, and for Deimos every third; the intermediate ones may be found by adding the periodic time of each satellite.

	h	m	s
Periodic time of Phobos	7	39	13·85
„ „ Deimos	30	17	54·86

Apparent Apsides.

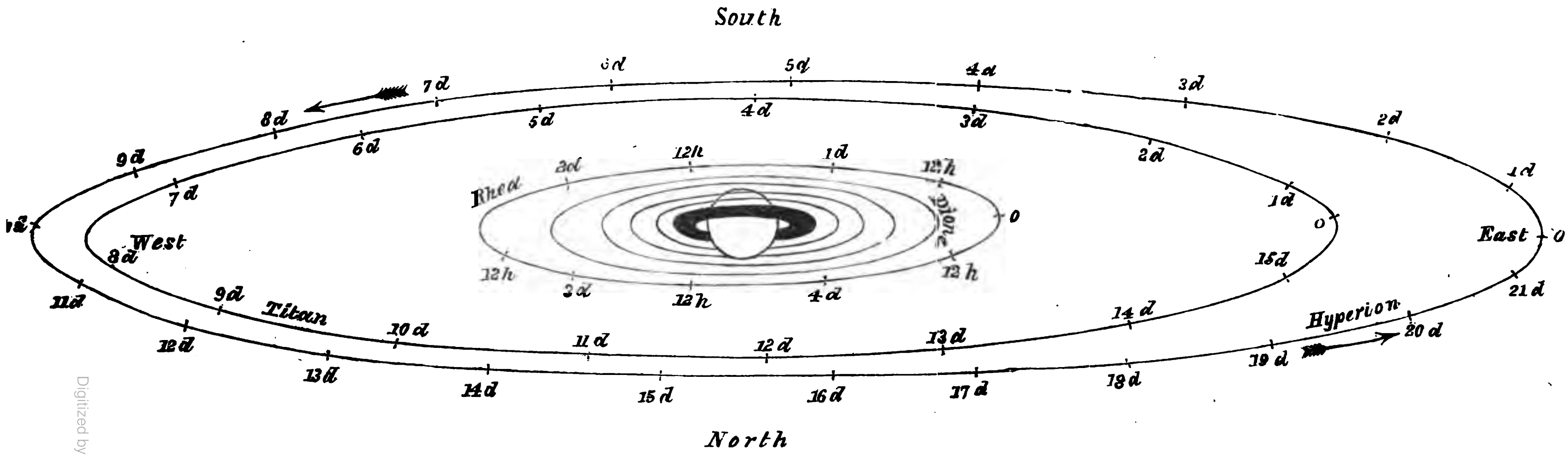
	Position-angle.	Apparent Distances.	
		Phobos.	Deimos.
	°	"	"
April 18	128·9	21·2	53·3
May 8	129·5	23·8	59·8
„ 28	129·7	23·6	59·3

Magnetic Elements, Royal Observatory, Greenwich.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1870, observed	19 53 west.	0·1782	67 52
1880, „	18 33 „	0·1804	67 36
1890, „	17 29 „	0·1823	67 23
1900, „	16 29 „	0·1844	67 8
1905, inferred	16 10 „	0·1856	66 55

The horizontal force is given in C. G. S. measure.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation

SATELLITES OF SATURN.

East Elongations.

MIMAS.

April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
14	0·1	1	21·9	1	1·6	1	5·3	1	7·5	1	9·7	1	13·4	1	15·8	1	19·7
14	22·7	2	20·5	2	0·2	2	3·9	2	6·1	2	8·4	2	12·0	2	14·4	2	18·3
15	21·3	3	19·1	2	22·9	3	2·5	3	4·7	3	7·0	3	10·6	3	13·0	3	16·9
16	19·9	4	17·7	3	21·5	4	1·1	4	3·4	4	5·6	4	9·3	4	11·6	4	15·5
17	18·6	5	16·3	4	20·1	4	23·8	5	2·0	5	4·2	5	7·9	5	10·3	5	14·2
18	17·2	6	15·0	5	18·7	5	22·4	6	0·6	6	2·8	6	6·5	6	8·9	6	12·8
19	15·8	7	13·6	6	17·3	6	21·0	6	23·2	7	1·4	7	5·1	7	7·5	7	11·4
20	14·4	8	12·2	7	15·9	7	19·6	7	21·8	8	0·0	8	3·7	8	6·1	8	10·0
21	13·0	9	10·8	8	14·5	8	18·2	8	20·4	8	22·6	9	2·3	9	4·7	9	8·7
22	11·7	10	9·4	9	13·2	9	16·8	9	19·1	9	21·3	10	1·0	10	3·4	10	7·3
23	10·3	11	8·1	10	11·8	10	15·4	10	17·7	10	19·9	10	23·6	11	2·0	11	5·9
24	8·9	12	6·7	11	10·4	11	14·0	11	16·3	11	18·5	11	22·2	12	0·6	12	4·5
25	7·5	13	5·3	12	9·0	12	12·7	12	14·9	12	17·1	12	20·8	12	23·2	13	3·2
26	6·1	14	3·9	13	7·6	13	11·3	13	13·5	13	15·7	13	19·4	13	21·8	14	1·8
27	4·8	15	2·5	14	6·2	14	9·9	14	12·1	14	14·3	14	18·0	14	20·5	15	0·4
28	3·4	16	1·1	15	4·9	15	8·5	15	10·7	15	13·0	15	16·6	15	19·1	15	23·0
29	2·0	16	23·7	16	3·5	16	7·1	16	9·3	16	11·6	16	15·3	16	17·7	16	21·6
30	0·6	17	22·3	17	2·1	17	5·7	17	7·9	17	10·2	17	13·9	17	16·3	17	20·3
30	23·2	18	21·0	18	0·7	18	4·3	18	6·5	18	8·8	18	12·5	18	15·0	18	18·9
		19	19·6	18	23·3	19	3·0	19	5·2	19	7·4	19	11·1	19	13·6	19	17·5
		20	18·2	19	21·9	20	1·6	20	3·8	20	6·0	20	9·7	20	12·2	20	16·1
		21	16·8	20	20·5	21	0·2	21	2·4	21	4·6	21	8·3	21	10·8	21	14·8
		22	15·4	21	19·1	21	22·8	22	1·0	22	3·2	22	6·9	22	9·4	22	13·4
		23	14·1	22	17·8	22	21·4	22	23·6	23	1·9	23	5·6	23	8·0	23	12·0
		24	12·7	23	16·4	23	20·0	23	22·2	24	0·5	24	4·2	24	6·7	24	10·6
		25	11·3	24	15·0	24	18·6	24	20·8	24	23·1	25	2·8	25	5·3	25	9·3
		26	9·9	25	13·6	25	17·2	25	19·4	25	21·7	26	1·4	26	3·9	26	7·9
		27	8·5	26	12·2	26	15·9	26	18·1	26	20·3	27	0·0	27	2·5	27	6·5
		28	7·1	27	10·8	27	14·5	27	16·7	27	19·0	27	22·7	28	1·1	28	5·1
		29	5·8	28	9·5	28	13·1	28	15·3	28	17·6	28	21·3	28	23·8	29	3·8
		30	4·4	29	8·1	29	11·7	29	13·9	29	16·2	29	19·9	29	22·4	30	2·4
		31	3·0	30	6·7	30	10·3	30	12·5	30	14·8	30	18·5	30	21·0	31	1·0
						31	8·9	31	11·1			31	17·2			31	23·7

ENCELADUS.

April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
14	5·3	2	0·9	1	4·4	1	7·8	1	20·0	2	8·1	1	2·5	1	14·9	1	18·5
15	14·2	3	9·8	2	13·3	2	16·6	3	4·8	3	17·0	2	11·4	2	23·7	3	3·4
16	23·1	4	18·6	3	22·2	4	1·5	4	13·7	5	1·8	3	20·3	4	8·6	4	12·3
18	7·9	6	3·5	5	7·0	5	10·4	5	22·6	6	10·7	5	5·2	5	17·5	5	21·2
19	16·8	7	12·4	6	15·9	6	19·3	7	7·5	7	19·6	6	14·0	7	2·4	7	6·1
21	1·7	8	21·3	8	0·8	8	4·2	8	16·3	9	4·4	7	22·9	8	11·3	8	15·0
22	10·6	10	6·2	9	9·7	9	13·0	10	1·2	10	13·3	9	7·8	9	20·2	9	23·9
23	19·5	11	15·1	10	18·6	10	21·9	11	10·1	11	22·2	10	16·7	11	5·1	11	8·8
25	4·4	13	0·0	12	3·4	12	6·8	12	19·0	13	7·1	12	1·6	12	14·0	12	17·7
26	13·3	14	8·9	13	12·3	13	15·7	14	3·8	14	16·0	13	10·4	13	22·9	14	2·6
27	22·2	15	17·8	14	21·2	15	0·6	15	12·7	16	0·8	14	19·3	15	7·8	15	11·5
29	7·1	17	2·7	16	6·1	16	9·4	16	21·6	17	9·7	16	4·2	16	16·6	16	20·4
30	16·0	18	11·6	17	15·0	17	18·3	18	6·5	18	18·6	17	13·1	18	1·5	18	5·3
		19	20·4	18	23·8	19	3·2	19	15·3	20	3·5	18	22·0	19	10·4	19	14·2
		21	5·3	20	8·7	20	12·1	21	0·2	21	12·4	20	6·9	20	19·3	20	23·0
		22	14·2	21	17·6	21	21·0	22	9·1	22	21·2	21	15·8	22	4·2	22	7·9
		23	23·1	23	2·5	23	5·8	23	18·0	24	6·1	23	0·7	23	13·1	23	16·8
		25	8·0	24	11·4	24	14·7	25	2·9	25	15·0	24	9·6	24	22·0	25	1·7
		26	16·9	25	20·2	25	23·6	26	11·7	26	23·9	25	18·5	26	6·9	26	10·6
		28	1·8	27	5·1	27	8·5	27	20·6	28	8·8	27	3·3	27	15·8	27	19·5
		29	10·6	28	14·0	28	17·4	29	5·5	29	17·6	28	12·2	29	0·7	29	4·4
		30	19·5	29	22·9	30	2·3	30	14·3			29	21·1	30	9·6	30	13·3
						31	11·1	31	23·2			31	6·0			31	22·2

East Elongations.

TETHYS.

April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
15	15·4	2	15·3	1	20·3	2	1·1	1	5·8	2	7·7	2	12·4	1	17·2	1	22·3
17	12·7	4	12·6	3	17·6	3	22·4	3	3·1	4	5·0	4	9·7	3	14·6	3	19·6
19	10·0	6	9·9	5	14·9	5	19·7	5	0·4	6	2·2	6	7·0	5	11·9	5	17·0
21	7·3	8	7·2	7	12·2	7	17·0	6	21·6	7	23·5	8	4·3	7	9·2	7	14·3
23	4·7	10	4·5	9	9·5	9	14·3	8	18·9	9	20·8	10	1·6	9	6·5	9	11·6
25	2·0	12	1·8	11	6·8	11	11·6	10	16·2	11	18·1	11	22·9	11	3·8	11	9·0
26	23·3	13	23·1	13	4·1	13	8·9	12	13·5	13	15·4	13	20·2	13	1·1	13	6·3
28	20·6	15	20·5	15	1·4	15	6·2	14	10·8	15	12·7	15	17·5	14	22·4	15	3·6
30	18·0	17	17·8	16	22·7	17	3·5	16	8·1	17	10·0	17	14·8	16	19·7	17	0·9
		19	15·1	18	20·0	19	0·8	18	5·4	19	7·3	19	12·1	18	17·0	18	22·2
		21	12·4	20	17·3	20	22·0	20	2·7	21	4·6	21	9·4	20	14·4	20	19·6
		23	9·7	22	14·6	22	19·3	22	0·0	23	1·9	23	6·7	22	11·7	22	16·9
		25	7·1	24	11·9	24	16·6	23	21·2	24	23·2	25	4·0	24	9·0	24	14·2
		27	4·4	26	9·2	26	13·9	25	18·5	26	20·5	27	1·3	26	6·3	26	11·5
		29	1·7	28	6·5	28	11·2	27	15·8	28	17·8	28	22·6	28	3·7	28	8·9
		30	23·0	30	3·8	30	8·5	29	13·1	30	15·1	30	19·9	30	1·0	30	6·2
								31	10·4								

DIONE.

April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
15	8·1	1	18·4	3	14·8	3	17·2	2	19·4	1	21·5	1	23·7	1	2·2	1	4·9
18	1·8	4	12·1	6	8·5	6	10·9	5	13·1	4	15·2	4	17·4	3	19·9	3	22·7
20	19·5	7	5·8	9	2·2	9	4·6	8	6·7	7	8·8	7	11·1	6	13·6	6	16·4
23	13·2	9	23·5	11	19·9	11	22·2	11	0·4	10	2·5	10	4·7	9	7·3	9	10·1
26	7·0	12	17·2	14	13·6	14	15·9	13	18·0	12	20·1	12	22·4	12	1·0	12	3·8
29	0·7	15	10·9	17	7·2	17	9·5	16	11·7	15	13·8	15	16·1	14	18·7	14	21·6
		18	4·6	20	0·9	20	3·2	19	5·3	18	7·4	18	9·8	17	12·4	17	15·3
		20	22·3	22	18·6	22	20·9	21	23·0	21	1·1	21	3·5	20	6·1	20	9·0
		23	16·1	25	12·2	25	14·5	24	16·6	23	18·8	23	21·1	22	23·8	23	2·7
		26	9·8	28	5·9	28	8·1	27	10·2	26	12·4	26	14·8	25	17·5	25	20·5
		29	3·4	30	23·6	31	1·8	30	3·9	29	6·1	29	8·5	28	11·2	28	14·2
		31	21·1													31	7·9

RHEA.

April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
17	1·5	5	3·5	1	6·3	2	21·1	3	11·5	4	1·7	1	3·7	1	18·4	3	9·6
21	14·0	9	16·0	5	18·7	7	9·5	7	23·8	8	14·0	5	16·0	6	6·8	7	22·2
26	2·5	14	4·5	10	7·2	11	21·8	12	12·1	13	2·3	10	4·4	10	19·3	12	10·7
30	15·0	18	17·0	14	19·6	16	10·2	17	0·4	17	14·6	14	16·8	15	7·7	16	23·2
		23	5·4	19	8·0	20	22·5	21	12·7	22	3·0	19	5·2	19	20·2	21	11·8
		27	17·9	23	20·4	25	10·8	26	1·0	26	15·3	23	17·6	24	8·7	26	0·3
				28	8·7	29	23·2	30	13·3			28	6·0	28	21·1	30	12·8

TITAN.

April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
20	3·2	6	3·0	7	1·6	8	22·3	9	17·5	10	12·3	12	8·0	13	5·1	15	4·1
		22	2·7	23	0·3	24	20·1	25	14·8	26	9·9	28	6·2	29	4·4	31	4·1

East Elongations.

HYPERION.

May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h
3 23·3	15 17·8	7 1·7	18 16·1	8 23·0	21 14·2	11 23·0	3 8·4
25 8·9		28 9·4		30 6·2			24 18·7

IAPETUS.

	d h	d h	d h	d h
E. Elongation ……	June 5 2·2,	Aug. 22 14·4,	Nov. 9 3·6	
Inf. Conjunction …	June 24 23·8,	Sept. 11 4·8,	Nov. 29 7·9	
W. Elongation ……	Apr. 26 16·3,	July 15 5·8,	Oct. 1 8·2,	Dec. 20 0·0
Sup. Conjunction …	May 16 17·0,	Aug. 3 15·8,	Oct. 20 20·9	

APPARENT ELEMENTS OF SATURN'S RINGS.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 4 ………	6 56·5	36·26	9·01	14 23·6 N.	13 2·6 N.
24 ………	6 50·4	35·77	8·31	13 26·3 N.	12 47·5 N.
Feb. 13 ………	6 43·3	35·62	7·65	12 24·7 N.	12 32·4 N.
Mar. 5 ………	6 35·7	35·80	7·06	11 22·9 N.	12 17·0 N.
25 ………	6 28·3	36·31	6·56	10 24·9 N.	12 1·6 N.
April 14 ………	6 21·6	37·13	6·18	9 34·9 N.	11 46·0 N.
May 4 ………	6 16·1	38·22	5·94	8 56·8 N.	11 30·4 N.
24 ………	6 12·6	39·50	5·88	8 34·0 N.	11 14·8 N.
June 13 ………	6 11·3	40·87	6·03	8 28·7 N.	10 59·1 N.
July 3 ………	6 12·5	42·18	6·37	8 41·2 N.	10 43·2 N.
23 ………	6 15·8	43·22	6·88	9 9·6 N.	10 27·3 N.
Aug. 12 ………	6 20·6	43·82	7·47	9 48·6 N.	10 11·2 N.
Sept. 1 ………	6 25·9	43·85	8·00	10 30·8 N.	9 55·1 N.
21 ………	6 30·6	43·30	8·36	11 7·7 N.	9 39·0 N.
Oct. 11 ………	6 33·8	42·28	8·46	11 32·2 N.	9 22·8 N.
31 ………	6 34·9	40·99	8·28	11 39·6 N.	9 6·5 N.
Nov. 20 ………	6 33·7	39·62	7·88	11 28·5 N.	8 50·1 N.
Dec. 10 ………	6 30·3	38·34	7·31	10 59·6 N.	8 33·6 N.
30 ………	6 24·8	37·28	6·64	10 15·7 N.	8 17·1 N.

SATELLITES OF URANUS.

North Elongations.

ARIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
16	7.9	1	10.9	3	5.4	3	11.5	2	17.7	1	23.8	2	5.7
18	20.4	3	23.4	5	17.9	6	0.0	5	6.2	4	12.3	4	18.2
21	8.9	6	11.9	8	6.4	8	12.6	7	18.7	7	0.8	7	6.7
23	21.4	9	0.4	10	18.9	11	1.1	10	7.2	9	13.3	9	19.2
26	9.9	11	12.9	13	7.4	13	13.6	12	19.7	12	1.8	12	7.7
28	22.4	14	1.4	15	19.9	16	2.1	15	8.3	14	14.3	14	20.1
		16	13.9	18	8.4	18	14.6	17	20.8	17	2.8		
		19	2.4	20	20.9	21	3.1	20	9.3	19	15.3		
		21	14.9	23	9.5	23	15.7	22	21.8	22	3.8		
		24	3.4	25	22.0	26	4.2	25	10.3	24	16.3		
		26	15.9	28	10.5	28	16.7	27	22.8	27	4.8		
		29	4.4	30	23.0	31	5.2	30	11.3	29	17.2		
		31	16.9										

UMBRIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
12	0.1	2	17.5	4	21.4	3	22.2	1	22.8	4	2.9	3	3.1
16	3.6	6	20.9	9	1.0	8	1.7	6	2.4	8	6.3	7	6.6
20	7.0	11	0.4	13	4.5	12	5.2	10	5.9	12	9.8	11	9.9
24	10.5	15	3.9	17	8.0	16	8.7	14	9.4	16	13.3	15	13.3
28	14.0	19	7.4	21	11.5	20	12.3	18	12.9	20	16.8	19	16.7
		23	10.9	25	15.1	24	15.8	22	16.4	24	20.2	23	20.1
		27	14.4	29	18.6	28	19.3	26	19.9	28	23.7	27	23.5
		31	17.9					30	23.4				

TITANIA.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
8	1.9	4	4.8	8	1.4	4	5.2	8	2.1	3	5.4	8	0.9
16	18.8	12	21.9	16	18.7	12	22.4	16	19.2	11	22.4	16	7.6
25	11.8	21	15.1	25	11.9	21	15.7	25	12.3	20	15.3	25	10.3
		30	8.2			30	8.9			29	8.1	November.	
												3	2.9

OBERON.

May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h
10	6.4	6	5.6	3	5.3	12	16.7	8	15.4	5	13.3
23	17.9	19	17.4	16	17.2	26	4.2	22	2.5	18	23.9
				30	5.0						

Apparent Apsides.

	Position-angle.	Apparent Distances.			
		Ariel.	Umbriel.	Titania.	Oberon.
	°	″	″	″	″
April 5	202.8	13.8	19.3	31.6	42.3
June 24	206.3	14.4	20.1	32.9	44.1
Sept. 12	210.3	13.8	19.2	31.6	42.2

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.		February.		March.		April.		September.		October.		November.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
1	10.6	5	17.3	1	5.7	5	12.1	5	5.0	4	13.9	2	23.1	2	8.6
7	7.7	11	14.4	7	2.8	11	9.1	11	1.9	10	10.9	8	20.2	8	5.7
13	4.8	17	11.5	12	23.9			16	22.9	16	8.0	14	17.3	14	2.8
19	2.0	23	8.6	18	20.9			22	19.9	22	5.0	20	14.4	19	23.9
24	23.1			24	18.0			28	16.9	28	2.1	26	11.5	25	21.1
30	20.2			30	15.0									31	18.2

Position-angle and distance of the apse:—Jan. 0, 268°.7, 16″.9; April 10, 267°.2, 16″.2; Sept. 22, 273°.1, 16″.2; Dec. 31, 271°.4, 16″.9.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1905.	P.	D.	L.	1905.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 2 1	−3 13	53 5	July 5	− 0 53	+3 29	131 16
6	− 0 26	3 47	347 13	10	+ 1 23	4 0	65 6
11	2 52	4 19	281 23	15	3 39	4 29	358 57
16	5 14	4 49	215 32	20	5 51	4 56	292 47
21	7 33	5 17	149 43	25	8 1	5 22	226 36
26	9 48	5 42	83 54	30	10 6	5 45	160 29
31	11 55	6 4	18 1	Aug. 4	12 5	6 5	94 22
Feb. 5	13 55	6 24	312 13	9	13 59	6 24	28 16
10	15 50	6 41	246 23	14	15 47	6 40	322 10
15	17 35	6 54	180 33	19	17 28	6 53	256 5
20	19 12	7 4	114 42	24	19 2	7 3	189 59
25	20 40	7 11	48 50	29	20 28	7 10	123 56
Mar. 2	21 59	7 15	342 59	Sept. 3	21 45	7 14	57 54
7	23 8	7 15	277 7	8	22 54	7 15	351 52
12	24 8	7 12	211 13	13	23 54	7 13	285 52
17	24 57	7 5	145 20	18	24 45	7 7	219 50
22	25 36	6 55	79 23	23	25 26	6 58	153 51
27	26 5	6 43	13 27	28	25 58	6 47	87 52
Apr. 1	26 23	6 28	307 29	Oct. 3	26 19	6 31	21 54
6	26 30	6 9	241 31	8	26 29	6 14	315 56
11	26 23	5 47	175 32	13	26 28	5 54	249 57
16	26 11	5 24	109 29	18	26 16	5 30	184 0
21	25 45	4 58	43 27	23	25 53	5 5	118 6
26	25 8	4 30	337 23	28	25 18	4 37	52 8
May 1	24 20	4 0	271 19	Nov. 2	24 31	4 6	346 12
6	23 21	3 29	205 14	7	23 32	3 34	280 15
11	22 11	2 56	139 6	12	22 20	3 0	214 20
16	20 51	2 22	72 59	17	20 59	2 25	148 26
21	19 22	1 47	6 51	22	19 26	1 48	82 32
26	17 44	1 11	300 41	27	17 42	1 11	16 38
31	15 56	−0 35	234 32	Dec. 2	15 48	+0 33	310 44
June 5	14 0	+0 1	168 21	7	13 46	−0 4	244 51
10	11 59	0 37	102 11	12	11 35	0 44	178 58
15	9 53	1 13	36 0	17	9 19	1 22	113 6
20	7 41	1 49	329 48	22	6 58	2 0	47 14
25	5 27	2 23	263 37	27	4 35	2 36	241 22
30	− 3 10	+2 57	197 27	32	2 8	−3 11	275 30

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 26′. In computing L (the heliographic longitude of the centre of the Sun's disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1905·0.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	V Sculptoris	0 3 49	−39 45·4	61.	R Cæli	4 37 12	−38 25·2
2.	S Sculptoris	0 10 34	−32 34·4	62.	R Pictoris	4 43 37	−49 25·0
3.	X Andromedæ	0 11 10	+46 29·0	63.	V Tauri	4 46 32	+17 22·7
4.	T Andromedæ	0 17 26	+26 28·1	64.	U Leporis	4 52 13	−21 22·1
5.	T Cassiopeiæ	0 18 5	+55 16·0	65.	R Orionis	4 53 51	+ 7 59·3
6.	S Toucani	0 18 38	−62 12·2	66.	S Aurigæ	4 55 9	+43 41·0
7.	R Andromedæ	0 19 1	+38 3·1	67.	R Leporis	4 55 17	−14 57·0
8.	S Ceti	0 19 13	− 9 51·3	68.	W Orionis	5 0 29	+ 1 2·8
9.	T Sculptoris	0 24 32	−38 26·0	69.	T Leporis	5 0 48	−22 2·0
10.	Y Cephei	0 31 39	+79 50·1	70.	V Orionis	5 1 3	+ 3 58·4
11.	U Cassiopeiæ	0 41 2	+47 44·3	71.	S Pictoris	5 8 26	−48 37·4
12.	V Andromedæ	0 44 56	+35 8·2	72.	R Aurigæ	5 9 37	+53 28·8
13.	X Sculptoris	0 44 56	−35 26·2	73.	T Columbæ	5 15 49	−33 48·4
14.	R^2 Andromedæ	0 46 13	+33 51·6	74.	W Aurigæ	5 20 29	+36 49·2
15.	W Cassiopeiæ	0 49 16	+58 2·9	75.	Y Aurigæ	5 21 53	+42 21·4
16.	U Cephei	0 53 49	+81 21·8	76.	S Orionis	5 24 19	− 4 46·1
17.	U Sculptoris	1 7 4	−30 37·2	77.	S Camelopardi	5 30 46	+68 44·8
18.	U Andromedæ	1 10 4	+40 13·0	78.	U Aurigæ	5 35 58	+31 59·6
19.	S Piscium	1 12 36	+ 8 25·8	79.	S Columbæ	5 43 22	−31 43·6
20.	S Cassiopeiæ	1 12 40	+72 6·7	80.	R Columbæ	5 46 52	−29 13·1
21.	U Piscium	1 17 57	+12 22·2	81.	Z Tauri	5 46 57	+15 46·3
22.	R Sculptoris	1 22 36	−33 2·1	82.	U^2 Tauri	5 47 9	+15 57·0
23.	R Piscium	1 25 44	+ 2 23·5	83.	V Camelopardi	5 50 3	+74 30·0
24.	U^2 Andromedæ	1 33 5	+38 11·0	84.	U Orionis	5 50 11	+20 9·5
25.	Y Andromedæ	1 34 3	+38 51·7	85.	Z Aurigæ	5 54 3	+53 18·1
26.	X Cassiopeiæ	1 50 6	+58 47·6	86.	R Octantis	5 55 16	−86 26·1
27.	U Persei	1 53 16	+54 21·6	87.	X Aurigæ	6 4 48	+50 15·0
28.	S Arietis	1 59 32	+12 4·3	88.	η Geminorum	6 9 9	+22 32·1
29.	R Arietis	2 10 43	+24 36·9	89.	V Aurigæ	6 16 41	+47 42·6
30.	W Andromedæ	2 11 33	+43 51·9	90.	V Monocerotis	6 17 56	− 2 8·9
31.	Z Cephei	2 13 27	+81 14·4	91.	T Monocerotis	6 20 6	+ 7 8·3
32.	o Ceti	2 14 33	− 3 24·5	92.	W Geminorum	6 29 31	+15 24·3
33.	R Ceti	2 21 10	− 0 36·4	93.	S Lyncis	6 36 22	+58 0·3
34.	S Horologii	2 22 31	−59 59·8	94.	X Geminorum	6 41 2	+30 22·3
35.	R Fornacis	2 25 0	−26 31·1	95.	W Monocerotis	6 47 45	− 7 1·9
36.	X Eridani	2 27 38	−41 52·8	96.	Y Monocerotis	6 51 36	+11 22·0
37.	U Ceti	2 29 10	−13 33·9	97.	R Lyncis	6 53 28	+55 27·9
38.	R^2 Cephei	2 30 4	+80 43·6	98.	S^2 Geminorum	6 55 33	+30 39·4
39.	R Trianguli	2 31 17	+33 51·0	99.	ζ Geminorum	6 58 29	+20 42·6
40.	Z Persei	2 33 59	+41 47·4	100.	R Geminorum	7 1 38	+22 51·1
41.	T Arietis	2 42 1	+17 6·8	101.	V Canis Min.	7 1 49	+ 9 1·1
42.	W Persei	2 43 35	+56 35·3	102.	R Canis Min.	7 3 29	+10 10·4
43.	R Horologii	2 50 43	−50 16·7	103.	R Volantis	7 7 21	−72 51·8
44.	T Horologii	2 57 50	−51 1·0	104.	L^2 Puppis	7 10 38	−44 29·3
45.	Algol	3 1 59	+40 35·4	105.	R^2 Monocerotis	7 12 42	+ 1 16·3
46.	U Arietis	3 5 47	+14 26·4	106.	R Canis Maj.	7 15 10	−16 12·9
47.	X Ceti	3 14 35	− 1 24·9	107.	R^2 Geminorum	7 15 31	+31 3·7
48.	Y Persei	3 21 15	+43 50·7	108.	V Geminorum	7 17 51	+13 16·5
49.	R Persei	3 24 0	+35 20·7	109.	U Monocerotis	7 26 16	− 9 34·6
50.	T Fornacis	3 25 37	−28 43·9	110.	S Canis Min.	7 27 34	+ 8 31·3
51.	U Eridani	3 46 27	−25 14·7	111.	Y Camelopardi	7 28 20	+76 16·3
52.	T Eridani	3 51 10	−24 18·6	112.	Z Puppis	7 28 31	−20 27·6
53.	λ Tauri	3 55 25	+12 13·4	113.	X Puppis	7 28 39	−20 42·3
54.	W Eridani	4 7 32	−25 22·7	114.	T Canis Min.	7 28 43	+11 56·4
55.	R Tauri	4 23 6	+ 9 57·1	115.	S Volantis	7 31 24	−73 10·5
56.	S Tauri	4 24 0	+ 9 44·2	116.	U Canis Min.	7 36 11	+ 8 36·2
57.	T Camelopardi	4 30 50	+65 57·4	117.	S Geminorum	7 37 21	+23 40·5
58.	R Reticuli	4 32 33	−63 13·6	118.	W Puppis	7 42 49	−41 57·8
59.	X Camelopardi	4 33 14	+74 56·6	119.	T Geminorum	7 43 36	+23 58·3
60.	R Doradûs	4 35 39	−62 15·8	120.	R^2 Puppis	7 43 41	−41 8·6

Mean Places of Variable Stars for 1905·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
121.	U Geminorum .	7 49 28	+22 15·1	181.	R Corvi	12 14 42	−18 43·
122.	U² Geminorum.	7 21 18	+21 37·8	182.	T Crucis	12 16 10	−61 45·
123.	V Puppis	7 55 31	−48 59·2	183.	R Crucis	12 18 25	−61 6·
124.	U Puppis	7 56 22	−12 34·6	184.	T Canum Ven. .	12 25 30	+32 1·
125.	S² Puppis	8 9 26	−34 17·5	185.	U Centauri ...	12 28 15	−54 8·
126.	Y Puppis	8 9 2	−34 51·3	186.	Y Virginis	12 28 58	− 3 53·
127.	R Cancri.........	8 11 20	+12 1·1	187.	T Ursæ Maj. ...	12 32 4	+60 0·
128.	V Cancri.........	8 16 18	+17 35·2	188.	R Virginis	12 33 41	+ 7 30·
129.	R Chamæleontis	8 23 56	−76 2·8	189.	R Muscæ..........	12 36 16	−68 53·
130.	V Carinæ	8 26 47	−59 48·3	190.	S Ursæ Maj. ...	12 39 48	+61 36·
131.	X Carinæ	8 29 13	−58 54·2	191.	U² Virginis......	12 42 28	+ 4 39·
132.	U Cancri.........	8 30 20	+19 13·4	192.	U Virginis	12 46 17	+ 6 4·
133.	X Ursæ Maj. ...	8 34 16	+50 28·0	193.	S Crucis	12 48 45	−57 54·
134.	T Velorum	8 34 35	−47 1·7	194.	T² Virginis	12 57 49	+ 5 41·
135.	S Cancri	8 38 31	+19 22·6	195.	U Octantis	13 12 46	−83 43·
136.	R Mali	8 41 30	−27 51·3	196.	W Virginis......	13 21 8	− 2 53·
137.	S Hydræ	8 48 37	+ 3 25·7	197.	V Virginis	13 22 54	− 2 40·
138.	T Hydræ.........	8 51 3	− 8 46·7	198.	R Hydræ	13 24 32	−22 47·
139.	T Cancri	8 51 15	+20 12·8	199.	S Virginis	13 28 2	− 6 42·
140.	S Mali	9 0 54	−24 42·6	200.	T Ursæ Minoris.	13 32 44	+73 54·
141.	V Ursæ Maj. ...	9 1 34	+51 29·8	201.	T Centauri	13 36 19	−33 7·
142.	W Cancri	9 4 20	+25 38·2	202.	T² Centauri ...	13 42 47	−36 23·
143.	W² Carinæ	9 18 16	−68 21·3	203.	W Hydræ	13 43 40	−27 53·
144.	V Velorum	9 19 24	−55 33·2	204.	R Canum Ven. .	13 44 52	+40 0·
145.	Y Velorum	9 25 50	−51 45·9	205.	X² Centauri ...	13 45 50	−36 28·
146.	S Antliæ	9 28 9	−28 12·5	206.	T Apodis.........	13 46 35	−77 20·
147.	S Velorum	9 29 38	−44 47·3	207.	R² Virginis......	13 59 51	− 8 44·
148.	U Velorum	9 29 39	−45 5·6	208.	Z Boötis	14 0 54	+13 57·
149.	R Carinæ	9 29 52	−62 22·1	209.	Z Virginis	14 5 14	−12 51·
150.	X Hydræ	9 30 58	−14 16·0	210.	U² Hydræ	14 6 5	−28 26·
151.	Y Draconis	9 31 40	+78 16·9	211.	R Centauri	14 9 43	−59 28·
152.	W Ursæ Maj. ...	9 37 5	+56 23·2	212.	R² Centauri......	14 10 16	−57 24·
153.	R Leo. Min. ...	9 39 54	+34 57·0	213.	Y Boötis	14 17 36	+20 14·
154.	R² Hydræ	9 40 38	−23 34·9	214.	S Boötis	14 19 42	+54 14·
155.	R Leonis	9 42 27	+11 52·2	215.	S² Virginis	14 22 31	+ 5 6·
156.	*l* Carinæ.........	9 42 38	−62 4·2	216.	R Camelopardi .	14 24 43	+84 15·
157.	Z Velorum	9 49 35	−53 43·9	217.	Y Centauri	14 25 23	−29 40·
158.	V Leonis	9 54 45	+21 42·9	218.	V Centauri	14 25 44	−56 28·
159.	R² Carinæ	9 55 0	−58 24·5	219.	V Boötis	14 25 55	+39 17·
160.	V² Carinæ	9 55 41	−63 26·5	220.	R Boötis	14 33 0	+27 8·
161.	S Carinæ.........	10 6 21	−61 5·1	221.	V Libræ	14 35 4	−17 14·
162.	Z Carinæ	10 10 37	−58 23·0	222.	S Lupi............	14 47 4	−46 13·
163.	W Velorum ...	10 11 42	−54 0·4	223.	U Boötis	14 49 56	+18 4·
164.	R² Velorum ...	10 18 0	−41 52·6	224.	δ Libræ	14 55 54	− 8 8·
165.	Y Carinæ	10 29 36	−58 0·5	225.	S Apodis.........	14 59 51	−71 41·
166.	Z² Carinæ	10 32 53	−70 13·0	226.	T² Libræ	15 1 4	−18 22·
167.	R Ursæ Maj. ...	10 37 56	+69 16·5	227.	T Libræ	15 5 19	−19 39·
168.	S² Hydræ	10 46 48	−28 7·7	228.	Y Libræ	15 6 40	− 5 39·
169.	V Hydræ	10 47 0	−20 44·7	229.	R Triang. Austr.	15 11 16	−66 8·
170.	W Leonis	10 48 37	+14 13·3	230.	U Coronæ	15 14 19	+31 59·
171.	U Carinæ	10 53 56	−59 13·4	231.	S Libræ	15 15 55	−20 2·
172.	S Leonis	11 5 56	+ 5 58·6	232.	S Serpentis	15 17 13	+14 39·
173.	S² Centauri......	11 16 19	−61 21·0	233.	S Coronæ	15 17 32	+31 42·
174.	Z Draconis	11 40 6	+72 47·4	234.	S² Libræ	15 18 47	−22 34·
175.	Z Hydræ	11 42 52	−32 44·5	235.	U² Libræ.........	15 27 58	−15 0·
176.	X Centauri	11 44 27	−41 13·6	236.	R Normæ	15 29 7	−49 11·
177.	W Centauri ...	11 50 17	−58 43·5	237.	X Libræ	15 30 43	−20 50·
178.	R Comæ	11 59 23	+19 18·9	238.	W Libræ.........	15 32 29	−15 51·
179.	S Muscæ	12 7 40	−69 37·4	239.	S Ursæ Minoris.	15 33 15	+78 57·
180.	T Virginis	12 9 44	− 5 30·5	240.	U Normæ	15 35 0	−55 0·

Mean Places of Variable Stars for 1905·0 (continued).

No.	Star.	R.A.			Dec.	
		h	m	s	°	′
241.	U Libræ	15	36	31	−20	52·5
242.	T Normæ	15	36	44	−54	41·0
243.	Z Libræ	15	40	59	−20	49·5
244.	V Coronæ	15	46	8	+39	51·4
245.	R Serpentis	15	46	19	+15	25·3
246.	R Lupi	15	47	18	−36	0·9
247.	R Libræ	15	48	13	−15	57·2
248.	R^2 Libræ	15	50	56	−18	1·5
249.	S Triang. Austr.	15	52	39	−63	30·3
250.	U Lupi	15	54	48	−29	39·1
251.	U Triang. Austr.	15	58	52	−62	39·1
252.	Z^2 Scorpii	15	58	55	−23	50·3
253.	X Herculis	15	59	48	+47	29·9
254.	Z Scorpii	16	0	26	−21	28·5
255.	R^2 Herculis	16	1	37	+50	45·4
256.	R Herculis	16	1	57	+18	37·6
257.	U Serpentis	16	2	44	+10	11·2
258.	X Scorpii	16	2	57	−21	16·3
259.	W Scorpii	16	6	13	−19	53·4
260.	U^2 Herculis	16	6	15	+25	19·1
261.	S Normæ	16	10	59	−57	39·9
262.	R Scorpii	16	11	59	−22	42·7
263.	S Scorpii	16	12	0	−22	39·5
264.	W Coronæ	16	12	1	+38	2·0
265.	W Ophiuchi	16	16	18	− 7	28·4
266.	V Ophiuchi	16	21	26	−12	12·7
267.	U Herculis	16	21	36	+19	6·5
268.	Y Scorpii	16	24	7	−19	14·0
269.	T Ophiuchi	16	28	18	−15	55·8
270.	S Ophiuchi	16	28	47	−16	57·6
271.	R Aræ	16	31	51	−56	48·2
272.	W Herculis	16	31	51	+37	32·0
273.	R Draconis	16	32	24	+66	57·1
274.	T^3 Scorpii	16	34	32	−32	11·6
275.	S Draconis	16	40	52	+55	5·9
276.	R^2 Ophiuchi	16	43	28	−19	17·9
277.	S Herculis	16	47	34	+15	6·1
278.	S^2 Scorpii	16	48	44	−44	56·9
279.	R^2 Scorpii	16	50	34	−30	25·7
280.	V^2 Scorpii	16	52	7	−33	27·7
281.	V^2 Herculis	16	56	56	+31	21·8
282.	R Ophiuchi	17	2	19	−15	58·0
283.	T^2 Herculis	17	6	59	+27	10·4
284.	W^2 Scorpii	17	8	38	−33	19·4
285.	U Ophiuchi	17	11	42	+ 1	18·9
286.	Z Ophiuchi	17	14	43	+ 1	37·0
287.	S^2 Herculis	17	17	44	+23	0·8
288.	S Octantis	17	28	6	−86	46·2
289.	U^2 Scorpii	17	35	34	−43	42·2
290.	X Sagittarii	17	41	34	−27	47·7
291.	U^3 Scorpii	17	41	56	−35	40·1
292.	Y^2 Scorpii	17	44	36	−33	40·6
293.	Y Ophiuchi	17	47	33	− 6	7·2
294.	W^2 Ophiuchi	17	50	44	+ 7	50·9
295.	S Aræ	17	51	51	−49	25·3
296.	T^2 Ophiuchi	17	52	5	+11	11·0
297.	Z Herculis	17	53	50	+15	8·3
298.	T Draconis	17	54	57	+58	13·6
299.	Y^2 Herculis	17	55	38	+19	29·3
300.	V Draconis	17	56	23	+54	52·7
301.	W Sagittarii	17	58	58	−29	35·2
302.	R Pavonis	18	3	45	−63	38·1
303.	T Herculis	18	5	30	+31	0·2
304.	S^2 Sagittarii	18	11	18	−34	8·5
305.	V Serpentis	18	11	21	−15	33·3
306.	W Lyræ	18	11	38	+36	38·4
307.	Y Sagittarii	18	15	48	−18	54·2
308.	V^2 Sagittarii	18	21	41	−33	22·7
309.	*d* Serpentis	18	22	21	+ 6	8·4
310.	T Serpentis	18	24	11	+12	14·0
311.	X^2 Herculis	18	26	14	+31	32·8
312.	U Sagittarii	18	26	18	−19	11·7
313.	Z^2 Herculis	18	32	56	+25	58·1
314.	X Ophiuchi	18	33	50	+ 8	44·7
315.	Y Lyræ	18	34	22	+43	52·1
316.	U Coron. Austr.	18	34	38	−37	55·4
317.	Z Coron. Austr.	18	34	47	−37	34·3
318.	Z^2 Lyræ	18	40	5	+32	41·9
319.	Y^2 Lyræ	18	41	26	+34	34·3
320.	W^2 Lyræ	18	42	16	+43	32·2
321.	R Scuti	18	42	25	− 5	48·5
322.	V Scuti	18	42	48	−12	14·1
323.	β Lyræ	18	46	34	+33	15·1
324.	κ Pavonis	18	47	10	−67	21·2
325.	U Scuti	18	49	8	−12	43·4
326.	X^2 Lyræ	18	50	38	+32	42·6
327.	R Lyræ	18	52	26	+43	49·2
328.	R Coron. Austr.	18	55	29	−37	5·3
329.	Z Lyræ	18	56	11	+34	49·4
330.	T^2 Lyræ	18	57	57	+37	23·1
331.	R Aquilæ	19	1	48	+ 8	5·2
332.	V Lyræ	19	5	22	+29	30·3
333.	X^2 Sagittarii	19	9	0	−18	58·3
334.	U^2 Lyræ	19	9	15	+41	8·6
335.	S Lyræ	19	9	19	+25	50·5
336.	S^2 Lyræ	19	9	29	+33	15·1
337.	U Draconis	19	9	57	+67	7·4
338.	W Aquilæ	19	10	16	− 7	12·5
339.	T Sagittarii	19	10	45	−17	8·2
340.	R Sagittarii	19	11	7	−19	28·5
341.	V^2 Lyræ	19	12	42	+32	15·2
342.	S Sagittarii	19	13	52	−19	11·8
343.	Z Sagittarii	19	14	5	−21	6·1
344.	U Sagittæ	19	14	39	+19	26·2
345.	U Lyræ	19	16	48	+37	42·1
346.	T Sagittæ	19	17	28	+17	28·4
347.	R^2 Lyræ	19	22	27	+42	36·1
348.	U Aquilæ	19	24	15	− 7	14·4
349.	V^4 Cygni	19	30	2	+28	6·9
350.	U Vulpeculæ	19	32	29	+20	7·3
351.	T^2 Aquilæ	19	33	34	+11	30·4
352.	R Cygni	19	34	16	+49	59·2
353.	V^2 Aquilæ	19	36	11	+ 9	42·1
354.	T Pavonis	19	40	4	−72	0·4
355.	T^2 Cygni	19	40	58	+48	32·6
356.	T^3 Cygni	19	41	1	+29	2·1
357.	X^3 Cygni	19	42	55	+32	28·3
358.	R^4 Cygni	19	43	27	+48	50·0
359.	S Vulpeculæ	19	44	30	+27	3·0
360.	X Aquilæ	19	46	46	+ 4	13·3

Mean Places of Variable Stars for 1905·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
361.	χ Cygni	19 46 55	+32 40·4	421.	T Octantis	20 58 16	−82 28·8
362.	S Pavonis	19 47 12	−59 26·5	422.	R Vulpeculæ	21 0 10	+23 26·7
363.	η Aquilæ	19 47 38	+ 0 45·7	423.	X^{5} Cygni	21 0 37	+39 35·5
364.	R^{2} Sagittarii	19 50 2	−29 26·2	424.	T^{4} Cygni	21 1 58	+29 1·5
365.	S Sagittæ	19 51 42	+16 22·9	425.	V Capricorni	21 2 5	−24 18·5
366.	U^{2} Sagittarii	19 52 11	−42 6·1	426.	U^{5} Cygni	21 2 31	+45 23·8
367.	R^{2} Aquilæ	19 52 40	− 2 8·4	427.	X Capricorni	21 3 7	−21 43·8
368.	S^{2} Aquilæ	19 53 59	− 8 8·5	428.	X Cephei	21 3 18	+82 41·0
369.	Z Cygni	19 58 46	+49 46·7	429.	Z Capricorni	21 5 20	−16 34·0
370.	Z^{5} Cygni	20 0 46	+41 19·0	430.	S^{2} Aquarii	21 6 0	− 4 24·9
371.	S Cygni	20 3 30	+57 42·7	431.	T Cephei	21 8 17	+68 6·3
372.	V^{3} Cygni	20 3 59	+46 1·5	432.	R Equulei	21 8 39	+12 24·6
373.	R Capricorni	20 5 59	−14 33·1	433.	R^{2} Aquarii	21 10 5	− 3 17·4
374.	S Aquilæ	20 7 15	+15 20·3	434.	X Pegasi	21 16 29	+14 2·9
375.	W^{2} Aquilæ	20 7 30	+15 46·6	435.	T Capricorni	21 16 46	−15 33·6
376.	R Telescopii	20 8 4	−47 17·1	436.	S Microscopii	21 21 6	−30 15·7
377.	U^{2} Aquilæ	20 8 17	+12 42·6	437.	Y Capricorni	21 29 12	−14 23·7
378.	Z^{2} Sagittarii	20 8 51	−44 41·7	438.	W Cygni	21 32 27	+44 57·0
379.	W Capricorni	20 8 54	−22 16·1	439.	S Cephei	21 36 25	+78 11·8
380.	R Sagittæ	20 9 44	+16 26·3	440.	U^{2} Cygni	21 37 26	+53 53·7
381.	S^{2} Cygni	20 9 56	+38 26·4	441.	R^{3} Cygni	21 38 59	+43 9·0
382.	Z Aquilæ	20 10 7	− 6 26·5	442.	V^{2} Cygni	21 39 21	+37 34·9
383.	R Delphini	20 10 20	+ 8 48·0	443.	R^{2} Pegasi	21 40 12	+24 34·2
384.	T^{2} Sagittarii	20 11 25	−39 24·3	444.	R Gruis	21 42 24	−47 21·2
385.	V^{5} Cygni	20 11 32	+34 12·7	445.	Y^{5} Cygni	21 47 53	+42 41·3
386.	V Sagittæ	20 15 59	+20 48·2	446.	T^{5} Cygni	21 55 26	+43 53·3
387.	U Cygni	20 16 40	+47 35·6	447.	V Pegasi	21 56 17	+ 5 39·8
388.	Z^{4} Cygni	20 19 47	+42 55·9	448.	U Aquarii	21 58 9	−17 5·1
389.	W^{2} Cygni	20 25 23	+39 39·8	449.	S Piscis Austr.	21 58 20	−28 30·6
390.	U^{2} Capricorni	20 26 59	−22 0·6	450.	T^{2} Pegasi	22 0 2	+34 39·7
391.	Z Delphini	20 28 18	+17 7·7	451.	T Pegasi	22 4 15	+12 4·5
392.	Y^{3} Cygni	20 29 48	+46 16·6	452.	Y Pegasi	22 7 1	+13 53·8
393.	S^{3} Cygni	20 30 3	+54 38·6	453.	S^{2} Pegasi	22 7 39	+14 5·1
394.	V Vulpeculæ	20 32 29	+26 16·4	454.	R Piscis Austr.	22 12 36	−30 4·7
395.	W Delphini	20 33 20	+17 57·1	455.	X Aquarii	22 13 26	−21 22·5
396.	R Microscopii	20 34 16	−29 7·6	456.	T Gruis	22 20 9	−38 2·9
397.	S Capricorni	20 36 11	−19 43·7	457.	S Gruis	22 20 15	−48 54·9
398.	Y Delphini	20 37 6	+11 32·0	458.	S Lacertæ	22 24 51	+39 49·8
399.	V Cygni	20 38 14	+47 48·1	459.	δ Cephei	22 25 38	+57 55·7
400.	S Delphini	20 38 42	+16 44·8	460.	R Indi	22 29 15	−67 46·7
401.	Y Aquarii	20 39 25	− 5 11·0	461.	W Cephei	22 32 50	+57 56·2
402.	X Cygni	20 39 41	+35 14·6	462.	T Toucani	22 34 18	−62 2·9
403.	T Delphini	20 40 57	+16 3·2	463.	R Lacertæ	22 39 3	+41 52·4
404.	W Aquarii	20 41 26	− 4 25·8	464.	S Aquarii	22 52 1	−20 51·0
405.	V Aquarii	20 42 1	+ 2 5·4	465.	R Pegasi	23 1 53	+10 1·8
406.	R^{2} Cygni	20 42 47	+44 30·8	466.	V Cassiopeiæ	23 7 35	+59 10·1
407.	U Capricorni	20 42 51	−15 8·0	467.	W Pegasi	23 15 1	+25 45·1
408.	V Delphini	20 43 27	+18 59·1	468.	S Pegasi	23 15 44	+ 8 24·0
409.	T Aquarii	20 44 56	− 5 30·0	469.	R Aquarii	23 38 54	−15 48·6
410.	T Vulpeculæ	20 47 27	+27 53·4	470.	Z Aquarii	23 47 20	−16 23·0
411.	U Pavonis	20 47 37	−63 4·1	471.	R^{2} Cassiopeiæ	23 51 2	+53 11·8
412.	Y Cygni	20 48 17	+34 18·0	472.	R Phœnicis	23 51 32	−50 18·9
413.	Z^{2} Cygni	20 48 42	+46 59·8	473.	V Cephei	23 51 57	+82 39·7
414.	S Indi	20 49 21	−54 41·2	474.	R Toucani	23 52 28	−65 54·7
415.	X Delphini	20 50 32	+17 16·8	475.	V Ceti	23 53 3	− 9 29·4
416.	R^{5} Cygni	20 51 7	+30 3·1	476.	U Pegasi	23 53 8	+15 25·6
417.	S^{5} Cygni	20 52 29	+30 4·0	477.	R Cassiopeiæ	23 53 34	+50 51·6
418.	W^{5} Cygni	20 53 46	+39 48·6	478.	S Phœnicis	23 54 10	−57 6·2
419.	U^{4} Cygni	20 56 36	+42 13·6	479.	W Ceti	23 57 15	−15 12·3
420.	R^{2} Capricorni	20 56 41	−27 27·8	480.	Y Cassiopeiæ	23 58 25	+55 9·0

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after the Star's name refers to Catalogue on pp. 24–27.)

Note.—The times of minima only are given; the times of maxima may be found by adding the interval printed under the name of the star.

T Monocer. 91.

($7^d\ 22\frac{1}{2}^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
19	11	7	13	19	14
Feb.		June		Oct.	
15	12	3	13	16	14
		30	13		
Mar.		July		Nov.	
14	12	27	13	12	15
Apr.		Aug.		Dec.	
10	12	23	14	9	15

W Geminor. 92*.

(Min. $7^d\ 18^h$.
Max. $2^d\ 15\frac{1}{2}^h$.)

Jan.		Apr.		Oct.	
d	h	d	h	d	h
2	21	5	18	8	13
18	9	21	6	24	0
		May		Nov.	
Feb.		6	18	8	12
2	20			23	23
18	8	Aug.			
		7	15		
		23	2	Dec.	
				9	11
Mar.		Sept.		24	23
5	19	7	14		
21	7	23	1		

ζ Geminor. 99.

($5^d\ 0\frac{1}{2}^h$.)

Jan.		Mar.		May	
d	h	d	h	d	h
4	6	6	4	6	2
14	9	16	7	16	6
24	13	26	11	26	9
Feb.		Apr.		June	
3	17	5	15	5	13
13	20	15	18		
24	0	25	22		

ζ Geminor. 99 (*continued*).

Aug.		Oct.		Dec.	
d	h	d	h	d	h
5	11	5	9	5	7
15	15	15	13	15	11
25	18	25	17	25	15
Sept.		Nov.			
4	22	4	20		
15	2	15	0		
25	6	25	4		

V Carinæ 130*.

(Min. $6^d\ 16\frac{1}{2}^h$.
Max. $2^d\ 4^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
6	21	7	10	4	22
20	7	20	19	18	7
Feb.		June		Oct.	
2	16	3	4	1	17
16	1	16	14	15	2
		29	23	28	11
Mar.					
1	11	July		Nov.	
14	20	13	8	10	21
28	6	26	18	24	6
Apr.		Aug.		Dec.	
10	15	9	3	7	15
24	0	22	13	21	1

T Velorum 134*.

(Min. $4^d\ 15^h{\cdot}3$.
Max. $1^d\ 9\frac{1}{2}^h$.)

Jan.		Mar.		May	
d	h	d	h	d	h
3	18	9	16	4	8
13	0	18	23	13	15
22	7	28	6	22	22
31	14				
				June	
Feb.		Apr.		1	4
9	20	6	12	10	11
19	3	15	19	19	18
28	10	25	2	29	1

T Velorum 134 (*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
8	7	1	23	5	22
17	14	11	6	15	5
26	21	20	13	24	11
		29	19		
				Dec.	
Aug.		Oct.		3	18
5	3	9	2	13	1
14	10	18	9	22	7
23	17	27	15	31	14

V Velorum 144*.

(Min. $4^d\ 8^h{\cdot}9$.
Max. $0^d\ 23\frac{1}{2}^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	5	4	14	4	0
10	23	13	8	12	17
19	17	22	2	21	11
28	11	30	20	30	5
Feb.		June		Oct.	
6	4	8	14	8	23
14	22	17	7	17	17
23	16	26	1	26	10
Mar.		July		Nov.	
4	10	4	19	4	4
13	4	13	13	12	22
21	21	22	7	21	16
30	15	31	0	30	10
Apr.		Aug.		Dec.	
8	9	8	18	9	3
17	3	17	12	17	21
25	21	26	6	26	15

S Muscæ 179*.

(Min. $9^d\ 15\frac{1}{2}^h$.
Max. $3^d\ 11^h$.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
5	10	13	1	4	8
24	17			23	16

S Muscæ 179 (*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
12	0	17	13	2	19
				22	3
May		Aug.			
1	7	5	21	Nov.	
20	15	25	4	10	10
				29	18
June		Sept.			
8	22	13	12	Dec.	
28	6			19	1

T Crucis 182*.

(Min. $6^d\ 17\frac{1}{2}^h$.
Max. $2^d\ 1\frac{1}{2}^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
5	4	6	9	4	13
18	16	19	20	18	0
Feb.		June		Oct.	
1	3	2	7	1	11
14	14	15	18	14	22
28	1	29	5	28	10
Mar.		July		Nov.	
13	12	12	16	10	31
26	23	26	4	24	8
Apr.		Aug.		Dec.	
9	10	8	15	7	19
22	22	22	2	21	6

R Crucis 183*.

(Min. $5^d\ 19^h{\cdot}8$.
Max. $1^d\ 9\frac{1}{2}^h$.)

Jan.		Mar.		May	
d	h	d	h	d	h
3	16	2	22	11	20
15	8	14	14	23	11
26	23	26	5		
Feb.		Apr.		June	
7	15	6	21	4	3
19	7	18	13	15	19
		30	4	27	10

R Crucis 183 (*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
9	2	5	8	2	14
20	17	16	23	14	5
		28	15	25	21
Aug.		Oct.		Dec.	
1	9	10	6	7	12
13	1	21	22	19	4
24	16			30	20

S Crucis 193*.

(Min. $4^d\ 16^h{\cdot}6$.
Max. $1^d\ 12^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
4	7	6	6	5	4
13	16	15	15	14	13
23	2	25	0	23	23
Feb.		June		Oct.	
1	11	3	9	3	8
10	20	12	18	12	17
20	5	22	3	22	2
				31	11
Mar.		July			
1	14	1	12	Nov.	
10	23	10	22	9	20
20	8	20	7	19	5
29	17	29	6	28	14
Apr.		Aug.		Dec.	
8	2	8	1	7	23
17	12	17	10	17	9
26	21	26	19	26	18

W Virginis 196.

($8^d\ 4\frac{1}{2}^h$.)

Jan.		May		Aug.	
d	h	d	h	d	h
16	21	17	19	12	3
				29	10
Feb.					
3	4	June		Sept.	
20	10			15	16
		4	1		
Mar.		21	8	Nov.	
9	17			6	12
26	23			23	18
Apr.		July		Dec.	
13	6	8	14	11	1
30	12	25	21	28	7

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* Alternate minima only are given: the others can be readily found by adding the interval under the name of the star.

VARIABLES OF SHORT PERIOD (*continued*).

V CENTAURI 218*.

(Min. 5^d 11^h·9. Max. 1^d 11½^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	13	3	10	1	7
13	13	14	10	12	6
24	12	25	9	23	6
Feb.		June		Oct.	
4	12	5	9	4	6
15	12	16	9	15	6
26	12	27	8	26	5
Mar.		July		Nov.	
9	11	8	8	6	5
20	11	19	8	17	5
31	11	30	8	28	4
Apr.		Aug.		Dec.	
11	10	10	7	9	4
22	10	21	7	20	4
				31	3

R TRIANG. AUSTR. 229*.

(Min. 3^d 9^h·3. Max. 1^d 0½^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	3	4	3	3	4
8	22	10	22	9	22
15	16	17	17	16	17
22	11	24	11	23	12
29	6	31	6	30	6
Feb.		June		Oct.	
5	0	7	1	7	1
11	19	13	19	13	20
18	14	20	14	20	14
25	8	27	9	27	9
Mar.		July		Nov.	
4	3	4	3	3	4
10	22	10	22	9	22
17	16	17	17	16	17
24	11	24	11	23	12
31	6	31	6	30	6
Apr.		Aug.		Dec.	
7	1	7	1	7	1
13	19	13	19	13	20
20	14	20	14	20	14
27	9	27	9	27	9

S TRIANG. AUSTR. 249*.

(Min. 6^d 7^h·8. Max. 2^d 2½^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
3	5	9	16	13	3
15	20	22	7	25	18
28	12				
Feb.		June		Oct.	
10	3	3	23	8	10
22	19	16	14	21	1
		29	6		
Mar.		July		Nov.	
7	10	11	21	2	17
20	2	24	13	15	8
				28	0
Apr.		Aug.		Dec.	
1	17	6	4	10	15
14	9	18	20	23	7
27	0	31	11		

S NORMÆ 261*.

(Min. 9^d 18^h. Max. 4^d 9½^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
3	7	19	19	13	20
22	19				
Feb.		June		Oct.	
11	7	8	8	3	8
		27	20	22	20
Mar.		July		Nov.	
2	19	17	8	11	8
22	7			30	21
Apr.		Aug.		Dec.	
10	19	5	20	20	9
30	7	25	8		

V² SCORPII 280*.

(Min. 6^d 1^h·5. Max. 1^d 9½^h.)

Jan.		Mar.		May	
d	h	d	h	d	h
3	21	5	12	5	2
16	0	17	15	17	5
28	3	29	18	29	8
Feb.		Apr.		June	
9	6	10	20	10	11
21	9	22	23	22	14

V² SCORPII 280.

(*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
4	17	3	8	2	23
16	20	15	11	15	2
28	23	27	14	27	5
Aug.		Oct.		Dec.	
10	2	9	17	9	8
22	5	21	20	21	11

X SAGITTAR. 290*

(Min. 7^d 0½^h. Max. 2^d 21^h.)

Feb.		May		Sept.	
d	h	d	h	d	h
5	17	28	21	4	1
19	17			18	2
Mar.		June		Oct.	
5	18	11	22	2	2
19	18	25	22	16	3
				30	3
Apr.		July		Nov.	
2	19	9	23	13	4
16	19	23	23	27	5
30	20				
May		Aug.			
14	21	7	0		
		21	1		

Y OPHIUCHI 293.

(6^d 5^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
9	3	8	23	5	19
26	6	26	2	22	22
Feb.		June		Oct.	
12	8	12	5	10	1
		29	8	27	4
Mar.		July		Nov.	
1	11	16	11	13	7
18	14			30	10
Apr.		Aug.		Dec.	
4	17	2	13	17	13
21	20	19	16		

W SAGITTAR. 301*.

(Min. 7^d 14½^h. Max. 3^d 0^h.)

Jan.		May		Aug.	
d	h	d	h	d	h
28	4	14	12	28	20
		29	17		
Feb.		June		Sept.	
12	9	13	21	13	0
27	13	29	2	28	5
Mar.		July		Oct.	
14	18	14	6	13	10
29	23	29	11	28	14
Apr.		Aug.		Nov.	
14	3	13	15	12	19
29	8			27	23

Y SAGITTAR. 307*.

(Min. 5^d 18½^h. Max. 2^d 2½^h.)

Jan.		May		Aug.	
d	h	d	h	d	h
30	10	2	19	14	17
		14	8	26	6
		25	21		
Feb.		June		Sept.	
10	23	6	11	6	19
22	12	18	0	18	9
		29	13	29	21
Mar.		July		Oct.	
6	2	11	2	11	10
17	15	22	15	23	0
29	4				
Apr.		Aug.		Nov.	
9	17	3	4	49	8
21	6			20	21

U SAGITTAR. 312*.

(Min. 6^d 17^h·9. Max. 2^d 23½^h.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
29	4	11	16	10	15
		25	4	24	3

U SAGITTAR. 312

(*continued*).

Apr.		June		Sept.	
d	h	d	h	d	h
6	15	26	13	2	0
20	3			15	12
				28	23
May		July		Oct.	
3	14	10	1	12	11
17	2	23	13	25	23
30	14				
June		Aug.		Nov.	
13	2	6	1	8	11
		19	12	21	22

β LYRÆ 323.

(Max. 3^d 2^h. Secondary Min. 6^d 11^h. Max. 9^d 18^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
4	14	13	19	7	1
17	13	26	17	19	23
30	11				
Feb.		June		Oct.	
12	9	8	15	2	21
25	7	21	13	15	19
				28	18
Mar.		July		Nov.	
10	5	4	11	10	16
23	3	17	9	23	14
		30	7		
Apr.		Aug.		Dec.	
5	1	12	5	6	12
17	23	25	3	19	10
30	21				

κ PAVONIS 324*.

(Min. 9^d 2½^h. Max. 4^d 9^h.)

Jan.		Mar.		May	
d	h	d	h	d	h
6	15	2	5	13	22
24	20	20	9		
Feb.		Apr.		June	
12	0	7	13	1	3
		25	18	19	7

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* Alternate minima only are given: the others can be readily found by adding the interval under the name of the star.

VARIABLES OF SHORT PERIOD (*continued*).

κ PAVONIS 324

(*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
7	12	18	6	11	19
25	16			30	0
Aug.		Oct.		Dec.	
12	21	6	10	18	5
31	1	24	15		

U AQUILÆ 348 *.

(Min. $7^{d}\ 0\frac{1}{2}^{h}$.
Max. $2^{d}\ 4\frac{1}{2}^{h}$.)

Feb.		May		Aug.	
d	h	d	h	d	h
6	10	29	19	22	2
20	11				
Mar.		June		Sept.	
6	12	12	20	5	3
20	14	26	22	19	5
Apr.		July		Oct.	
3	15	10	23	3	6
17	16	25	0	17	7
				31	8
May		Aug.		Nov.	
1	17	8	1	14	9
15	18			28	10

U VULPEC. 350 *.

(Min. $7^{d}\ 23\frac{1}{2}^{h}$.
Max. $2^{d}\ 3^{h}$.)

Feb.		Apr.		June	
d	h	d	h	d	h
2	23	7	19	10	15
18	22	23	18	26	14
Mar.		May		July	
6	21	9	17	12	13
22	20	25	16	28	12

U VULPEC. 350*

(*continued*).

Aug.		Sept.		Nov.	
d	h	d	h	d	h
13	11	30	9	1	7
29	10			17	6
Sept.		Oct.		Dec.	
14	10	16	8	3	5
				19	4

T^3 CYGNI 356 *.

(Min. $3^{d}\ 20^{h}\cdot 3$.
Max. $1^{d}\ 8^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	21	4	23	5	1
9	14	12	15	12	17
17	6	20	8	20	10
24	23	28	1	28	2
Feb.		June		Oct.	
1	16	4	17	5	19
9	8	12	10	13	12
17	1	20	3	21	4
24	17	27	19	28	21
Mar.		July		Nov.	
4	10	5	12	5	13
12	3	13	4	13	6
19	19	20	21	20	23
27	12	28	14	28	15
Apr.		Aug.		Dec.	
4	4	5	6	6	8
11	21	12	23	14	1
19	14	20	15	21	17
27	6	28	8	29	10

η AQUILÆ 363 *.

(Min. $7^{d}\ 4^{h}$.
Max. $2^{d}\ 9^{h}$.)

Feb.		Mar.		Apr.	
d	h	d	h	d	h
1	1	1	18	13	19
15	9	16	2	28	4
		30	11		

η AQUILÆ 363*

(*continued*).

May		Aug.		Oct.	
d	h	d	h	d	h
12	12	6	15	31	18
26	21	20	23		
June		Sept.		Nov.	
10	5	4	8	15	2
24	14	18	16	29	11
July		Oct.		Dec.	
8	22	3	1	13	19
23	6	17	9	28	4

S SAGITTÆ 365*.

(Min. $8^{d}\ 9\frac{1}{2}^{h}$.
Max. $3^{d}\ 10^{h}$.)

Feb.		May		Sept.	
d	h	d	h	d	h
8	21	20	12	14	21
25	16				
Mar.		June		Oct.	
14	10	6	6	1	15
31	5	23	1	18	9
Apr.		July		Nov.	
16	23	9	19	4	4
		26	13	20	22
May		Aug.		Dec.	
3	17	12	8	7	17
		29	2		

X CYGNI 402.

($5^{d}\ 3^{h}$.)

Jan.		Apr.		July	
d	h	d	h	d	h
7	18	16	1	6	23
24	3			23	9
Feb.		May		Aug.	
9	12	2	10	8	18
25	21	18	20	25	3
Mar.		June		Sept.	
14	7	4	5	10	12
30	16	20	14	26	21

X CYGNI 402

(*continued*).

Oct.		Nov.		Dec.	
d	h	d	h	d	h
13	7	15	1	1	10
29	16			17	20

T VULPEC. 410*.

(Min. $4^{d}\ 10\frac{1}{2}^{h}$.
Max. $1^{d}\ 9\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	19	5	23	7	4
10	15	14	20	16	1
19	12	23	17	24	22
28	9				
Feb.		June		Oct.	
6	6	1	14	3	19
15	3	10	11	12	16
24	0	19	8	21	13
		28	5	30	10
Mar.		July		Nov.	
4	21	7	2	8	7
13	18	15	23	17	4
22	15	24	20	26	0
31	12				
Apr.		Aug.		Dec.	
9	9	2	17	4	21
18	6	11	13	13	18
27	2	20	10	22	15
		29	7	31	12

U^4 CYGNI 419.

($5^{d}\ 3^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	11	14	1	8	22
16	5	28	19	23	16
30	22				
Feb.		June		Oct.	
14	16	12	13	8	9
		27	6	23	3
Mar.		July		Nov.	
1	9	12	0	6	21
16	3	26	17	21	14
30	21				
Apr.		Aug.		Dec.	
14	14	10	11	6	8
29	8	25	5	21	1

X^5 CYGNI 423*.

(Min. $7^{d}\ 20\frac{1}{2}^{h}$.
Max. $2^{d}\ 2^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	14	8	7	11	0
18	7	24	0	26	17
Feb.		June		Oct.	
3	0	8	17	12	10
18	17	24	10	28	3
Mar.		July		Nov.	
6	10	10	3	12	20
22	3	25	20	28	13
Apr.		Aug.		Dec.	
6	20	10	14	14	7
22	14	26	7	30	0

δ CEPHEI 459 *.

(Min. $5^{d}\ 9^{h}$.
Max. $1^{d}\ 9\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
6	4	4	5	10	0
16	22	14	23	20	18
27	15	25	16		
Feb.		June		Oct.	
7	9	5	10	1	11
18	2	16	3	12	5
28	20	26	21	22	22
Mar.		July		Nov,	
11	13	7	15	2	16
22	7	18	8	13	9
		29	2	24	3
Apr.		Aug.		Dec.	
2	1	8	19	4	21
12	18	19	13	15	14
23	12	30	6	26	8

* Alternate minima only are given: the others can be readily found by adding the interval under the name of the star.

MAXIMA OF S^5 CYGNI 417.

Jan.			Feb.			March.			April.			May.			June.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
0	20	3	5	17	20	4	15	18	9	12	36	6	10	34	6	20	12
9	19	22	14	16	40	13	14	38	18	11	55	15	9	53	15	19	31
18	18	42	23	15	59	22	13	57	27	11	15	19	21	33	24	18	50
27	18	1				31	13	16				28	20	52			

July.			August.			Sept.			Oct.			Nov.			Dec.		
3	18	10	8	15	27	4	13	25	1	11	23	6	8	41	3	6	39
12	17	29	17	14	47	13	12	45	10	10	43	15	8	0	12	5	58
21	16	48	26	14	6	22	12	4	19	10	2	24	7	19	21	5	18
30	16	8							28	9	21				30	4	37

Every sixteenth maximum is given; the others may be found by adding multiples of the period $13^{h}\ 27^{m}{\cdot}5$. The minimum takes place $1^{h}\ 55^{m}$ before the maximum.

MAXIMA OF U PEGASI 476.

Jan.			Feb.			April.			May.			June.			July.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
1	0	50	3	18	22	4	17	33	4	17	8	3	16	43	3	16	18
4	8	47	7	12	19	8	11	29	8	11	5	7	10	40	7	10	15
8	12	44	11	6	16	12	5	26	12	5	1	11	4	37	11	4	12
12	6	41	15	0	13	15	23	23	15	22	58	14	22	33	14	22	9
16	0	38	18	18	10	19	17	20	19	16	55	18	16	30	18	16	5
19	18	35	22	12	7	23	11	17	23	10	52	22	10	27	22	10	2
23	12	32	26	6	4	27	5	14	27	4	49	26	4	24	26	3	59
27	6	29				30	23	11	40	22	46	29	22	21	29	21	56
31	0	25															

August.			Sept.			Oct.			Nov.			Dec.		
2	15	53	1	15	28	1	15	3	4	8	35	4	8	10
6	9	50	5	9	25	5	9	0	8	2	32	8	2	7
10	3	47	9	3	22	9	2	57	11	20	29	11	20	4
13	21	44	12	21	19	12	20	54	15	14	26	15	14	1
17	15	41	16	15	16	16	14	51	19	8	23	19	7	58
21	9	37	20	9	13	20	8	48	23	2	20	23	1	55
25	3	34	24	3	9	24	2	45	27	20	17	26	19	52
28	21	31	27	21	6	27	20	41	30	14	13	30	13	49
						31	14	38						

Every twentieth maximum is given; the others may be found by adding multiples of the period $4^{h}\ 29^{m}{\cdot}8$. The minimum takes place $2^{h}\ 15^{m}$ after the maximum.

MAXIMA OF Z^2 LYRÆ 318.

Jan.			Feb.			March.			April.			May.			June.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
1	9	15	1	1	30	3	17	45	3	10	0	4	2	15	3	18	30
6	11	58	6	4	13	8	20	28	8	12	43	9	4	58	8	21	13
11	14	40	11	6	55	13	23	10	13	15	25	14	7	40	13	23	55
16	17	23	16	9	38	19	1	53	18	18	8	19	10	23	19	2	58
21	20	5	21	12	20	24	4	35	23	20	50	24	13	5	24	5	20
26	22	48	26	15	3	29	7	18	28	23	33	29	15	48	29	8	3

July.			August.			Sept.			Oct.			Nov.			Dec.		
4	10	45	4	3	0	3	19	15	4	11	30	4	3	45	4	20	0
9	13	28	9	5	43	8	21	58	9	14	13	9	6	28	9	22	43
14	16	10	14	8	25	14	0	40	14	16	55	14	9	10	15	1	25
19	18	53	19	11	8	19	3	23	19	19	38	19	11	53	20	4	8
24	21	35	24	13	50	24	6	5	24	22	20	24	14	35	25	6	50
30	0	18	29	16	33	29	8	48	30	1	3	29	17	18	30	9	38

Every tenth maximum is given; the others may be found by adding multiples of the period $12^{h}\ 16^{m}{\cdot}2$.

MAXIMA OF Y^3 CYGNI 392.

Jan.		Feb.		Mar.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
1	20	1	0	3	4	2	8	2	12	1	16	1	20	1	0	15	6	15	10	14	14	14	18
16	22	16	2	18	6	17	10	17	14	16	18	16	22	16	2	30	8	30	12	29	16	29	20
														31	4								

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following the Star's name are the Nos. in Catalogue, pp. 24–27.)

U Cephei 16. *Fourth* *. $2^{d}\ 11^{h}\ 49^{m}{\cdot}7$.

	d	h	m
Jan.	1	17	30
	11	16	49
	21	16	8
	31	15	27
Feb.	10	14	46
	20	14	5
March.	2	13	24
	12	12	43
	22	12	2
April.	1	11	21
	11	10	40
	21	9	59
May.	1	9	18
	11	8	37
	21	7	56
	31	7	15
June.	10	6	34
	20	5	53
	30	5	12
July.	10	4	31
	20	3	50
	30	3	9
August.	9	2	27
	19	1	46
	29	1	5
Sept.	8	0	24
	17	23	43
	27	23	2
Oct.	7	22	21
	17	21	40
	27	20	59
Nov.	6	20	18
	16	19	37
	26	18	56
Dec.	6	18	15
	16	17	34
	26	16	53

Z Persei 40. *Fourth.* $3^{d}\ 1^{h}\ 21^{m}{\cdot}5$.

	d	h	m
Jan.	1	17	0
	13	22	26
	26	3	52
Feb.	7	9	18
	19	14	44
March.	3	20	10
	16	1	36
	28	7	3
April.	9	12	29
	21	17	55
May.	3	23	21
	16	4	47
	28	10	13
June.	9	15	39
	21	21	6
July.	4	2	32
	16	7	58
	28	13	24
August.	9	18	50
	22	0	16
Sept.	3	5	43
	15	11	9
	27	16	35
Oct.	9	22	1
	22	3	27
Nov.	3	8	53
	15	14	19
	27	19	46
Dec.	10	1	12
	22	6	38

Algol 45. *Third.* $2^{d}\ 20^{h}\ 49^{m}$.

	d	h	m
Jan.	2	11	21
	11	1	48
	19	16	14
	28	6	50
Feb.	5	21	8
	14	11	35
	23	2	2
March.	3	16	29
	12	6	56
	20	21	23
	29	11	50
April.	7	2	16
	15	16	43
	24	7	10
June.	14	21	52
	23	12	18
July.	2	2	45
	10	17	12
	19	7	39
	27	22	6
August.	5	12	33
	14	3	0
	22	17	27
	31	7	54
Sept.	8	22	20
	17	12	47
	26	3	14
Oct.	4	17	41
	13	8	8
	21	22	35
	30	13	2
Nov.	8	3	29
	16	17	56
	25	8	22
Dec.	3	22	49
	12	13	16
	21	3	43
	29	18	10

T^2 Persei. *Ninth.* $0^{d}\ 20^{h}\ 23^{m}{\cdot}2$.

	d	h	m
Jan.	1	1	40
	8	17	9
	16	8	37
	24	0	6
	31	15	35
Feb.	8	7	3
	15	22	32
	23	14	1
March.	3	5	29
	10	20	58
	18	12	27
	26	3	55
April.	2	19	24
	10	10	53
	18	2	21
	25	17	50
May.	3	9	18
	11	0	47
	18	16	16
	26	7	44
June.	2	23	13
	10	14	42
	18	6	10
	25	21	39
July.	3	13	8
	11	4	36
	18	20	5
	26	11	34
August.	3	3	2
	10	18	31
	18	10	0
	26	1	28
Sept.	2	16	57
	10	8	26
	17	23	54
	25	15	23
Oct.	3	6	51
	10	22	20
	18	13	49
	26	5	17
Nov.	2	20	46
	10	12	15
	18	3	43
	25	19	12
Dec.	3	10	41
	11	2	9
	18	17	38
	26	9	7

λ Tauri 53. *Second.* $3^{d}\ 22^{h}\ 52^{m}$.

	d	h	m
Jan.	4	16	49
	12	14	33
	20	12	18
	28	10	2
Feb.	5	7	47
	13	5	31
	21	3	15
March.	1	1	0
	8	22	44
	16	20	29
	24	18	13
April.	1	15	57
	9	13	42
	17	11	26
	25	9	11
July.	1	13	58
	9	11	42
	17	9	27
	25	7	11
August.	2	4	56
	10	2	40
	18	0	24
	25	22	9
Sept.	2	19	53
	10	17	38
	18	15	22
	26	13	6
Oct.	4	10	51
	12	8	35
	20	6	20
	28	4	4
Nov.	5	1	48
	12	23	33
	20	21	17
	28	19	2
Dec.	6	16	46
	14	14	30
	22	12	15
	30	9	59

R Canis Min. 102. *Sixth.* $1^{d}\ 3^{h}\ 15^{m}{\cdot}8$.

	d	h	m
Jan.	0	16	18
	7	11	52
	14	7	27
	21	3	1
	27	22	36
Feb.	3	18	11
	10	13	45
	17	9	20
	24	4	54
March.	3	0	29
	9	20	4
	16	15	38
	23	11	13
	30	6	47
April.	6	2	22
	12	21	57
	19	17	31
	26	13	6
May.	3	8	40
	10	4	15
	16	23	50
	23	19	24
	30	14	59
Sept.	0	18	32
	7	14	6
	14	9	41
	21	5	15
	28	0	50
Oct.	4	20	25
	11	15	59
	18	11	34
	25	7	8
Nov.	1	2	43
	7	22	18
	14	17	52
	21	13	27
	28	9	1
Dec.	5	4	36
	12	0	11
	18	19	45
	25	15	20

Y Camelop. 111. *Second.* $3^{d}\ 7^{h}\ 20^{m}{\cdot}1$.

	d	h	m
Jan.	3	0	24
	9	15	4
	16	5	44
	22	20	24
	29	11	5
Feb.	5	1	45
	11	16	25
	18	7	5
	24	21	45
March.	3	12	26
	10	3	6
	16	17	46
	23	8	26
	29	23	6
April.	5	13	47
	12	4	27
	18	19	7
	25	9	47
May.	2	0	27
	8	15	8
	15	5	48
	21	20	28
	28	11	8
June.	4	1	48
	10	16	29
	17	7	9
	23	21	49
	30	12	29
July.	7	3	9
	13	17	50
	20	8	30
	26	23	10
August.	2	13	50
	9	4	30
	15	19	11
	22	9	51
	29	0	31
Sept.	4	15	11
	11	5	51
	17	20	32
	24	11	12
Oct.	1	1	52
	7	16	32
	14	7	12
	20	21	53
	27	12	33

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* The Second, Third, Fourth, or other minima are given in the columns as shown by the ordinal at the head. Times of the other minima can be found by adding multiples of the period given.

Minima of Variable Stars of the Algol Type (continued).

Y CAMELOP. 111 (*con.*).

	d	h	m
Nov.	3	3	13
	9	17	53
	16	8	33
	22	23	14
	29	13	54
Dec.	6	4	34
	12	19	14
	19	9	54
	26	0	35

R² PUPPIS 120. *Second.* $6^d\ 10^h\ 19^m\cdot5$.

	d	h	m
Jan.	2	11	29
	15	8	7
	28	4	45
Feb.	10	1	24
	22	22	2
March.	7	18	41
	20	15	19
April.	2	11	57
	15	8	36
	28	5	14
May.	11	1	53
	23	22	31
June.	5	19	9
	18	15	48
July.	1	12	26
	14	9	5
	27	5	43
August.	9	2	21
	21	23	0
Sept.	3	19	38
	16	16	17
	29	12	55
Oct.	12	9	33
	25	6	12
Nov.	7	2	50
	19	23	29
Dec.	2	20	7
	15	16	45
	28	13	24

V PUPPIS 123. *Eighth.* $1^d\ 10^h\ 54^m\cdot4$.

	d	h	m
Jan.	1	1	54
	12	17	9
	24	8	25
Feb.	4	23	40
	16	14	56
	28	6	12
March.	11	21	27
	23	12	43
April.	4	3	58
	15	19	14
	27	10	29
May.	9	1	45
	20	17	1
June.	1	8	16
	12	23	32
	24	14	47
July.	6	6	3
	17	21	18
	29	12	34
August.	10	3	49
	21	19	5
Sept.	2	10	21
	14	1	36
	25	16	52
Oct.	7	8	7
	18	23	23
	30	14	38
Nov.	11	5	54
	22	21	9
Dec.	4	12	25
	16	3	41
	27	18	56

S CANCRI 135.

	d	h	m
Jan.	3	20	48
	13	8	26
	22	20	3
Feb.	1	7	41
	10	19	19
	20	6	57
March.	1	18	34
	11	6	12
	20	17	50
	30	5	28
April.	8	17	5
	18	4	43
	27	16	21
May.	7	3	59
	16	15	36
	26	3	14
June.	4	14	52
	14	2	30
	23	14	7
July.	3	1	45
	12	13	23
	22	1	1
	31	12	38
Sept.	7	11	9
	16	22	47
	26	10	25
Oct.	5	22	3
	15	9	40
	24	21	18
Nov.	3	8	56
	12	20	34
	22	8	11
Dec.	1	19	49
	11	7	27
	20	19	5
	30	6	42

S ANTLIÆ 146. *Fortieth.* $0^d\ 7^h\ 46^m\cdot8$.

	d	h	m
Jan.	1	2	33
	14	1	46
	27	0	58
Feb.	9	0	10
	21	23	22
March.	6	22	34
	19	21	46
April.	1	20	59
	14	20	11
	27	19	23
May.	10	18	35
	23	17	47
June.	5	16	59
	18	16	11
July.	1	15	24
	14	14	36
	27	13	48
Sept.	4	11	24
	17	10	37
	30	9	49
Oct.	13	9	1
	26	8	13
Nov.	8	7	25
	21	6	37
Dec.	4	5	49
	17	5	2
	30	4	14

S VELORUM 147. *Second.* $5^d\ 22^h\ 24^m\cdot4$.

	d	h	m
Jan.	3	4	45
	15	1	33
	26	22	22
Feb.	7	19	11
	19	15	59
March.	3	12	48
	15	9	37
	27	6	26
April.	8	3	14
	20	0	3
May.	1	20	52
	13	17	40
	25	14	29
June.	6	11	18
	18	8	7
	30	4	55
July.	12	1	44
	23	22	33
August.	4	19	21
	16	16	10
	28	12	59
Sept.	9	9	47
	21	6	36
Oct.	3	3	25
	15	0	14
	26	21	2
Nov.	7	17	51
	19	14	40
Dec.	1	11	28
	13	8	17
	25	5	6

W URSÆ MAJ. 152. *Sixtieth.* $0^d\ 4^h\ 0^m\cdot2$.

	d	h	m
Jan.	0	7	49
	10	8	2
	20	8	14
	30	8	27
Feb.	9	8	40
	19	8	53
March.	1	9	6
	11	9	18
	21	9	31
	31	9	44
April.	10	9	57
	20	10	10
	30	10	22
May.	10	10	35
	20	10	48
	30	11	1
June.	9	11	14
	19	11	26
	29	11	39
July.	9	11	52
	19	12	5
	29	12	18
August.	8	12	30
	18	12	43
	28	12	56
Sept.	7	13	9
	17	13	22
	27	13	34
Oct.	7	13	47
	17	14	0
	27	14	13
Nov.	6	14	26
	16	14	38
	26	14	51
Dec.	6	15	4
	16	15	17
	26	15	30

R² VELORU 164. *Fourth.* $1^d\ 20^h\ 30^m\cdot2$

	d	h	m
Jan.	2	12	20
	9	22	20
	17	8	20
	24	18	20
Feb.	1	4	21
	8	14	21
	16	0	21
	23	10	21
March.	2	20	21
	10	6	21
	17	16	22
	25	2	22
April.	1	12	22
	8	22	22
	16	8	22
	23	18	22
May.	1	4	22
	8	14	23
	16	0	23
	23	10	23
	30	20	23
June.	7	6	23
	14	16	23
	22	2	24
	29	12	24
July.	6	22	24
	14	8	24
	21	18	24
	29	4	24
August.	5	14	25
	13	0	25
	20	10	25
	27	20	25
Sept.	4	6	25
	11	16	25
	19	2	26
	26	12	26
Oct.	3	22	26
	11	8	26
	18	18	26
	26	4	26
Nov.	2	14	27
	10	0	27
	17	10	27
	24	20	27
Dec.	2	6	27
	9	16	27
	17	2	27
	24	12	28
	31	22	28

S Antliæ: 20 periods $= 6^d\ 11^h\ 36^m\cdot1$.

W Ursæ Maj.: 30 periods $= 5^d\ 0^h\ 6^m\cdot4$.

Minima of Variable Stars of the Algol Type (continued).

Z Draconis 174. *Sixth.* $^{d}\ 8^{h}\ 34^{m}\cdot 7$.

	d	h	m
Jan.	1	2	50
	9	6	18
	17	9	46
	25	13	15
Feb.	2	16	43
	10	20	11
	18	23	39
	27	3	8
March.	7	6	36
	15	10	4
	23	13	33
	31	17	1
April.	8	20	29
	16	23	58
	25	3	26
May.	3	6	54
	11	10	22
	19	13	51
	27	17	19
June.	4	20	47
	13	0	16
	21	3	44
	29	7	12
July.	7	10	41
	15	14	9
	23	17	37
	31	21	5
August.	9	0	34
	17	4	2
	25	7	30
Sept.	2	10	59
	10	14	27
	18	17	55
	26	21	24
Oct.	5	0	52
	13	4	20
	21	7	48
	29	11	17
Nov.	6	14	45
	14	18	13
	22	21	42
Dec.	1	1	10
	8	4	38
	17	8	7
	25	11	35

δ Libræ 224. *Fourth.* $2^{d}\ 7^{h}\ 51^{m}\cdot 4$.

	d	h	m
Jan.	2	6	52
	11	14	17
	20	21	43
	30	5	8
Feb.	8	12	34
	17	19	59
	27	3	25
March.	8	10	50
	17	18	16
	27	1	41
April.	5	9	7
	14	16	32
	23	23	58
May.	3	7	23
	12	14	49
	21	22	14
	31	5	40
June.	9	13	5
	18	20	31
	28	3	56
July.	7	11	22
	16	18	47
	26	2	13
August.	4	9	38
	13	17	4
	23	0	30
Sept.	1	7	55
	10	15	21
	19	22	46
	29	6	12
Dec.	8	1	53
	17	9	19
	26	16	44

U Coronæ 230. *Third.* $3^{d}\ 10^{h}\ 51^{m}\cdot 2$.

	d	h	m
Jan.	2	13	27
	12	22	0
	23	6	34
Feb.	2	15	8
	12	23	41
	23	8	15
March.	5	16	49
	16	1	23
	26	9	56
April.	5	18	30
	16	3	4
	26	11	37
May.	6	20	11
	17	4	45
	27	13	19
June.	6	21	52
	17	6	26
	27	15	0
July.	7	23	34
	18	8	7
	28	16	41
August.	8	1	15
	18	9	48
	28	18	22
Sept.	8	2	56
	18	11	30
	28	20	3
Oct.	9	4	37
	19	13	11
	29	21	44
Nov.	9	6	18
	19	14	52
	29	23	26
Dec.	10	7	59
	20	16	33
	31	1	7

R Aræ 271. *Second.* $4^{d}\ 10^{h}\ 12^{m}\cdot 1$.

	d	h	m
Jan.	2	22	54
	11	19	18
	20	15	42
	29	12	6
Feb.	7	8	31
	16	4	45
	25	1	19
March.	5	21	43
	14	18	8
	23	14	32
April.	1	10	56
	10	7	20
	19	3	45
	28	0	9
May.	6	20	33
	15	16	58
	24	13	22
June.	2	9	46
	11	6	10
	20	2	35
	28	22	59
July.	7	19	23
	16	15	47
	25	12	12
August.	3	8	36
	12	5	0
	21	1	24
	29	21	49
Sept.	7	18	13
	16	14	37
	25	11	1
Oct.	4	7	26
	13	3	50
	22	0	14
	30	20	39
Nov.	8	17	3
	17	13	27
	26	9	51
Dec.	5	6	16
	14	2	40
	22	23	4
	31	19	28

U Ophiuchi 285. *Tenth.* $0^{d}\ 20^{h}\ 7^{m}\cdot 7$.

	d	h	m
Jan.	1	4	10
	9	13	27
	17	22	43
	26	8	0
Feb.	3	17	17
	12	2	34
	20	11	51
	28	21	8
March.	9	6	24
	17	15	41
	26	0	58
April.	3	10	15
	11	19	32
	20	4	49
	28	14	6
May.	6	23	22
	15	8	39
	23	17	56
June.	1	3	13
	9	12	30
	17	21	47
	26	7	3
July.	4	16	20
	13	1	37
	21	10	54
	29	20	11
August.	7	5	28
	15	14	44
	24	0	1
Sept.	1	9	18
	9	18	35
	18	3	52
	26	13	9
Oct.	4	22	26
	13	7	43
	21	16	59
	30	2	16
Nov.	7	11	33
	15	20	50

Z Herculis 297. *Fourth.*

	d	h	m
Jan.	2	1	1
	10	0	40
	18	0	19
	25	23	59
Feb.	2	23	38
	10	23	17
	18	22	56
	26	22	35
March.	6	22	14
	14	21	53
	22	21	33
	30	21	12
April.	7	20	51
	15	20	30
	23	20	9
May.	1	19	48
	9	19	27
	17	19	6
	25	18	46
June.	2	18	25
	10	18	4
	18	17	43
	26	17	22
July.	4	17	1
	12	16	40
	20	16	20
	28	15	59
August.	5	15	38
	13	15	17
	21	14	56
	29	14	35
Sept.	6	14	14
	14	13	53
	22	13	33
	30	13	12
Oct.	8	12	51
	16	12	30
	24	12	9
Nov.	1	11	48
	9	11	27
	17	11	6
	25	10	46
Dec.	3	10	25
	11	10	4
	19	9	43
	27	9	22

S² Sagittarii 304. *Fourth.* $2^{d}\ 9^{h}\ 58^{m}\cdot 6$.

	d	h	m
Jan.	25	14	20
Feb.	4	6	15
	13	22	9
	23	14	4
March.	5	5	58
	14	21	53
	24	13	47
April.	3	5	41
	12	21	36
	22	13	30
May.	2	5	25
	11	21	19
	21	13	14
	31	5	8
June.	9	21	3
	19	12	57
	29	4	51
July.	8	20	46
	18	12	40
	28	4	35
August.	6	20	29
	16	12	24
	26	4	18
Sept.	4	20	13
	14	12	7
	24	4	1
Oct.	3	19	56
	13	11	50
	23	3	45
Nov.	1	19	39
	11	11	34
	21	3	28
	30	19	23

Z Herculis: 1 period $= 1^{d}\ 22^{h}\ 28^{m}$; 2 periods $= 3^{d}\ 23^{h}\ 50^{m}$; 3 periods $= 5^{d}\ 22^{h}\ 17^{m}$.

Minima of Variable Stars of the Algol Type (continued).

V Serpentis 305. *Fourth.* 3d 10h 53m·0.

	d	h	m
Feb.	12	10	0
	26	5	32
March.	12	1	4
	25	20	36
April.	8	16	8
	22	11	40
May.	6	7	12
	20	2	44
June.	2	22	16
	16	17	48
	30	13	20
July.	14	8	52
	28	4	24
August.	10	23	56
	24	19	28
Sept.	7	15	0
	21	10	32
Oct.	5	6	4
	19	1	36
Nov.	1	21	8
	15	16	40

X² Herculis 311. *Twelfth.* 0d 21h 20m·5.

	d	h	m
Jan.	10	21	53
	21	14	0
Feb.	1	6	6
	11	22	13
	22	14	19
March.	5	6	26
	15	22	32
	26	14	39
April.	6	6	46
	16	22	52
	27	14	59
May.	8	7	5
	18	23	12
	29	15	19
June.	9	7	25
	19	23	32
	30	15	38
July.	11	7	45
	21	23	51
August.	1	15	58
	12	8	5
	23	0	11
Sept.	2	16	18
	13	8	24
	24	0	31
Oct.	4	16	37
	15	8	44
	26	0	51
Nov.	5	16	57
	16	9	4
	27	1	10
Dec.	7	17	17
	18	9	24

U² Lyræ 334. *Fourth.* 3d 14h 22m·4.

	d	h	m
Jan.	3	22	4
	18	7	33
Feb.	1	17	3
	16	2	32
March.	2	12	2
	16	21	32
	31	7	1
April.	14	16	31
	29	2	0
May.	13	11	30
	27	20	59
June.	11	6	29
	25	15	59
July.	10	1	28
	24	10	58
August.	7	20	27
	22	5	57
Sept.	5	15	26
	20	0	56
Oct.	4	10	26
	18	19	55
Nov.	2	5	25
	16	14	54
Dec.	1	0	24
	15	9	53
	29	19	23

U Sagittæ 344. *Fourth.* 3d 9h 8m·2.

	d	h	m
Jan.	0	23	12
	14	11	45
	28	0	17
Feb.	10	12	50
	24	1	23
March.	9	13	55
	23	2	28
April.	5	15	1
	19	3	33
May.	2	16	6
	16	4	39
	29	17	11
June.	12	5	44
	25	18	17
July.	9	6	49
	22	19-	22
August.	5	7	55
	18	20	27
Sept.	1	9	0
	14	21	33
	28	10	5
Oct.	11	22	38
	25	11	11
Nov.	7	23	43
	21	12	16
Dec.	5	0	49
	18	13	22

X³ Cygni 357. *Second.* 6d 0h 9m.

	d	h	m
Jan.	3	8	20
	15	8	38
	27	8	55
Feb.	8	9	13
	20	9	30
March.	4	9	48
	16	10	6
	28	10	23
April.	9	10	41
	21	10	58
May.	3	11	16
	15	11	34
	27	11	51
June.	8	12	9
	20	12	26
July.	2	12	44
	14	13	2
	26	13	19
August.	7	13	37
	19	13	54
	31	14	12
Sept.	12	14	30
	24	14	47
Oct.	6	15	5
	18	15	22
	30	15	40
Nov.	11	15	58
	23	16	15
Dec.	5	16	33
	17	16	50
	29	17	8

Z⁵ Cygni 370. *Third.* 3d 7h 38m.

	d	h	m
Jan.	3	2	39
	13	1	32
	23	0	26
Feb.	1	23	20
	11	22	13
	21	21	7
March.	3	20	1
	13	18	54
	23	17	48
April.	2	16	42
	12	15	36
	22	14	29
May.	2	13	23
	12	12	17
	22	11	10
June.	1	10	4
	11	8	58
	21	7	51
July.	1	6	45
	11	5	39
	21	4	33
	31	3	26
August.	10	2	20
	20	1	14
	30	0	7
Sept.	8	23	1
	18	21	55
	28	20	48
Oct.	8	19	42
	18	18	36
	28	17	30
Nov.	7	16	23
	17	15	17
	27	14	11
Dec.	7	13	4
	17	11	58
	27	10	52

V³ Cygni 372. *Second.* 4d 13h 45m.

	d	h	m
Jan.	3	22	12
	13	1	42
	22	5	12
	31	8	42
Feb.	9	12	12
	18	15	42
	27	19	12
March.	8	22	42
	18	2	12
	27	5	42
April.	5	9	12
	14	12	42
	23	16	13
May.	2	19	43
	11	23	13
	21	2	43
	30	6	13
June.	8	9	43
	17	13	13
	26	16	43
July.	5	20	13
	14	23	43
	24	3	13
August.	2	6	43
	11	10	13
	20	13	43
	29	17	13
Sept.	7	20	44
	17	0	14
	26	3	44
Oct.	5	7	14
	14	10	44
	23	14	14
Nov.	1	17	44
	10	21	14
	20	0	44
	29	4	14
Dec.	8	7	44
	17	11	14
	26	14	44

Minima of Variable Stars of the Algol Type (continued).

V^{5} Cygni 385.	Z^{4} Cygni 388.	W Delphini 395.	Y Cygni 412.	U^{5} Cygni 426.	T^{5} Cygni 446.
	Fourth.	*Second.*	*Sixth.*	*Sixth*	
	$3^{d}\ 10^{h}\ 49^{m}{\cdot}2$.	$4^{d}\ 19^{h}\ 21^{m}{\cdot}2$.		$1^{d}\ 11^{h}\ 28^{m}{\cdot}0$.	
Jan.	Jan.	Jan.	Jan.	Jan.	Jan.
d h m	d h m	d h m	d h m	d h m	d h m
9 1 22	1 3 31	1 20 2	1 8 6	1 3 53	25 3 2
17 11 45	14 22 48	11 10 44	10 7 51	10 0 35	Feb.
25 22 8	28 18 5	21 1 27	19 7 36	18 21 17	25 10 20
Feb.	Feb.	30 16 9	28 7 21	27 17 58	March.
3 8 31	11 13 22	Feb.	Feb.	Feb.	28 17 37
11 18 54	25 8 38	9 6 52	6 7 6	5 14 40	April.
20 5 17	March.	18 21 34	15 6 51	14 11 22	29 0 55
28 15 40	11 3 55	28 12 16	24 6 36	23 8 3	May.
March.	24 23 12	March.	March.	March.	30 8 13
9 2 3	April.	10 2 59	5 6 21	4 4 45	June.
17 12 26	7 18 29	19 17 41	14 6 6	13 1 27	30 15 31
25 22 49	21 13 45	29 8 24	23 5 51	21 22 9	July.
April.	May.	April.	April.	30 18 50	31 22 48
3 9 12	5 9 2	7 23 6	1 5 36	April.	Sept.
11 19 35	19 4 19	17 13 48	10 5 21	8 15 32	1 6 6
20 5 58	June.	27 4 31	19 5 6	17 12 14	Oct.
28 16 21	1 23 36	May.	28 4 51	26 8 56	2 13 24
May.	15 18 53	6 19 13	May.	May.	Nov.
7 2 44	29 14 9	16 9 56	7 4 36	5 5 37	2 20 42
15 13 7	July.	26 0 38	16 4 21	14 2 19	Dec.
23 23 30	13 9 26	June.	25 4 6	22 23 1	4 3 59
June.	27 4 43	4 15 20	June.	31 19 42	
1 9 53	August.	14 6 3	3 3 51	June.	
9 20 16	10 0 0	23 20 45	12 3 36	9 16 24	
18 6 39	23 19 16	July.	21 3 21	18 13 6	
26 17 2	Sept.	3 11 28	30 3 6	27 9 48	
July.	6 14 33	13 2 10	July.	July.	
5 3 25	20 9 50	22 16 52	9 2 51	6 6 29	
13 13 48	Oct.	August.	18 2 36	15 3 11	
22 0 11	4 5 7	1 7 35	27 2 22	23 23 53	
30 10 34	18 0 24	10 22 17	August.	August.	
August.	31 19 40	20 13 0	5 2 6	1 20 34	
7 20 57	Nov.	30 3 42	14 1 51	10 17 16	
16 7 20	14 14 57	Sept.	23 1 36	19 13 58	
24 17 43	28 10 14	8 18 24	Sept.	28 10 40	
Sept.	Dec.	18 9 7	1 1 22	Sept.	
2 4 6	12 5 31	27 23 49	10 1 7	6 7 21	
10 14 29	26 0 47	Oct.	19 0 52	15 4 3	
19 0 52		7 14 32	28 0 37	24 0 45	
27 11 15		17 5 14	Oct.	Oct.	
Oct.		26 19 56	7 0 22	2 21 26	
5 21 38		Nov.	16 0 7	11 18 8	
14 8 1		5 10 39	24 23 52	20 14 50	
22 18 24		15 1 21	Nov.	29 11 32	
31 4 47		24 16 4	2 23 37	Nov.	
Nov.		Dec.	11 23 22	7 8 13	
8 15 10		4 6 46	20 23 7	16 4 55	
17 1 33		13 21 28	29 22 52	25 1 37	
25 11 56		23 12 11	Dec.	Dec.	
Dec.			8 22 37	3 22 19	
3 22 19			17 22 22	12 19 0	
12 8 42			26 22 7	21 15 42	
20 19 5				30 12 24	
29 5 28					

Y Cygni: other minima follow those given by $1^{d}\ 13^{h}\ 44^{m}$, $2^{d}\ 23^{h}\ 54^{m}$, $4^{d}\ 13^{h}\ 39^{m}$, $5^{d}\ 23^{h}\ 50^{m}$, and 7^{d} [illegible] (in January) by

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to the Catalogue on pp. 24–27; M signifies maximum; *m*, minimum.

January.

d	No.	
0	65	M
	37	M
	52	M
	46	M
	298	*m*
	24	*m*
	214	*m*
	460	*m*
	404	*m*
1	22	M
	28	M
	180	M
	250	*m*
	327	*m*
3	380	*m*
	466	*m*
	171	*m*
5	265	M
	206	M
	70	M
6	299	M
	162	M
	266	M
7	181	M
	302	*m*
	303	*m*
8	472	*m*
	54	*m*
9	171	M
	190	M
	125	*m*
10	74	M
	260	M
	156	M
	292	*m*
11	68	*m*
12	276	M
	384	*m*
	374	*m*
13	479	M
	172	M
14	60	M
	474	M
	359	*m*
	242	*m*
15	47	M
16	108	M
	413	M
	55	M
	434	*m*
	394	*m*
17	227	M
	438	M
	292	M
	281	*m*
18	418	M
19	393	M

January (*con.*).

d	No.	
19	175	*m*
20	6	M
	380	M
	422	M
	210	M
22	283	M
	358	M
	125	M
	39	*m*
24	327	M
	94	M
	205	M
	253	M
25	306	*m*
	104	*m*
26	132	M
	154	M
	201	M
	50	*m*
	409	*m*
27	68	M
	391	*m*
	24	*m*
28	447	*m*
	109	*m*
29	456	*m*
	63	*m*
	167	*m*
30	148	M
	159	M
	8	*m*
31	59	M
	77	M
	11	M
	155	*m*

February.

d	No.	
1	292	*m*
2	184	M
	156	*m*
	149	*m*
3	98	M
	10	M
5	178	M
	26	M
	367	M
	221	*m*
	382	*m*
6	328	*m*
7	292	M
	418	M
8	56	M
	21	M
9	359	M
10	354	*m*
11	314	*m*
	171	*m*

February (*con.*).

d	No.	
11	161	*m*
12	33	M
	316	*m*
13	118	M
	458	M
	68	*m*
14	51	M
	186	*m*
15	187	M
	156	M
	109	M
	48	*m*
16	405	M
	171	M
	204	*m*
	88	*m*
	327	*m*
17	175	M
	277	*m*
18	467	M
	80	*m*
19	313	M
	287	M
20	73	M
	248	M
	464	M
	125	*m*
	292	*m*
	437	*m*
21	76	M
22	86	M
	289	M
	241	M
	335	*m*
23	317	M
	246	M
	263	M
	23	*m*
24	57	M
	20	M
	188	*m*
25	32	M
26	373	M
	259	*m*
	355	*m*
27	292	M
	418	M
	462	M
	244	M
	253	*m*
28	362	M
	233	M
	471	M

March.

d	No.	
1	68	M
	170	M

March (*con.*).

d	No.	
2	208	M
	185	*m*
3	150	M
	288	M
	148	*m*
4	255	M
5	125	M
	87	M
	257	M
	202	*m*
6	258	M
7	93	M
	178	*m*
	27	*m*
8	337	M
	223	*m*
9	50	M
	435	M
	156	*m*
10	96	M
11	327	M
	175	*m*
	100	*m*
12	95	M
	396	*m*
	433	*m*
	292	*m*
13	436	M
	442	*m*
14	478	M
	378	*m*
	237	*m*
	380	*m*
15	109	*m*
16	460	M
	457	M
	245	*m*
17	68	*m*
18	480	M
	328	M
	427	M
	273	*m*
19	366	M
	418	M
	292	M
20	300	M
21	166	M
	252	*m*
22	156	M
	171	*m*
	359	*m*
23	121	M
	278	M
	173	*m*
	49	*m*
24	61	M
	29	*m*

March (*con.*).

d	No.	
24	340	*m*
25	374	M
	104	M
26	415	*m*
	177	*m*
	438	*m*
27	54	M
	279	M
	171	M
	303	M
	364	*m*
28	231	*m*
	400	*m*
29	31	M
	465	*m*
30	421	*m*
	35	*m*
	201	*m*
31	191	M
	386	M
	234	M
	380	M

April.

d	No.	
1	85	M
	117	M
2	148	M
	342	M
	469	M
	129	M
	68	M
	109	M
	292	*m*
	394	*m*
	125	*m*
3	452	M
	422	*m*
4	274	M
	420	M
	330	M
	327	*m*
5	38	M
	448	M
	450	M
	24	M
6	44	M
7	302	M
	7	M
	137	*m*
8	418	M
	220	*m*
9	175	M
	292	M
	192	*m*
	25	*m*
10	37	*m*

April (*con.*).

d	No.	
10	286	*m*
11	382	M
12	456	M
	195	*m*
13	470	M
	199	*m*
14	156	*m*
15	316	M
	125	M
16	140	M
	463	M
	346	*m*
17	331	M
	349	*m*
18	359	M
	68	*m*
19	434	M
20	466	M
	172	*m*
	153	*m*
21	118	*m*
22	5	M
	9	*m*
	292	*m*
24	409	M
25	281	M
	1	M
	429	*m*
	272	*m*
26	306	M
	161	M
	201	M
	327	M
	379	*m*
27	156	M
28	353	M
	24	M
	418	M
	253	M
	267	M
	50	*m*
29	477	M
	292	M
	171	*m*
30	109	*m*

May.

d	No.	
1	398	M
	369	M
	63	M
	198	M
	343	*m*
	175	*m*
	47	*m*
2	124	M
	242	M

May (*con.*).

d	No.	
3	431	M
	188	M
	128	*m*
4	68	M
	148	*m*
5	171	M
	190	*m*
6	383	M
	328	*m*
7	214	M
	216	M
	110	*m*
	4	*m*
	3	*m*
8	310	M
	308	M
	102	M
	28	*m*
9	390	M
	30	M
	21	*m*
10	186	M
	264	M
	338	M
12	292	*m*
13	266	*m*
	125	*m*
15	396	M
17	89	M
	326	M
18	115	M
	418	M
	472	M
	371	M
	109	M
19	292	M
	167	M
	94	*m*
	156	*m*
20	475	M
	52	*m*
	33	*m*
	97	*m*
	327	*m*
21	354	M
	407	M
	68	*m*
22	384	M
	67	*m*
23	447	M
	197	M
24	421	M
	238	M
	380	*m*
25	39	M
	355	M
26	125	M

May (*con.*).

d	No.	
26	239	*m*
27	319	M
	90	*m*
	473	*m*
	70	*m*
29	438	M
	98	M
	87	*m*
	468	*m*
	359	*m*
30	175	M
	227	*m*
	176	*m*
31	228	M
	71	M
	163	M
	19	*m*
	219	*m*
	253	*m*

June.

d	No.	
1	160	M
	173	M
	156	M
	478	*m*
2	275	M
	414	M
	237	M
	292	*m*
3	148	M
	437	M
	58	M
4	252	M
	221	M
	422	M
5	346	M
	54	M
	425	M
	62	*m*
6	68	M
	358	*m*
7	418	M
	171	*m*
	374	*m*
8	149	M
	413	*m*
9	50	M
	292	M
	51	*m*
	108	*m*
10	380	M
	298	M
	48	M
11	223	M

June (*con.*).

d	No.	
11	378	M
	439	M
12	327	M
	115	M
	382	*m*
13	171	M
	247	M
14	118	M
	158	M
	2	*m*
	69	*m*
	104	*m*
15	329	M
	328	M
	456	*m*
	109	*m*
16	185	M
	203	*m*
	425	*m*
17	121	M
	335	M
18	458	*m*
	116	*m*
19	41	*m*
20	73	*m*
	246	*m*
21	424	M
	303	*m*
22	292	*m*
	68	*m*
23	18	M
	29	M
24	143	M
	177	M
	8	M
	155	M
	359	M
	125	*m*
	156	*m*
25	399	M
	430	M
	184	*m*
26	15	M
	368	M
	292	M
27	49	M
	418	M
	78	M
	43	*m*
28	12	M
29	194	M
	292	M
	231	M
	201	*m*
30	404	M
	234	*m*
	427	*m*

Maxima and Minima of Variable Stars (continued).

July

d	No.
2	59 M
3	136 M
	168 M
	202 M
	270 M
	109 M
	360 *m*
	6 *m*
4	217 M
	441 M
	207 M
	273 M
	10 *m*
5	386 M
	62 M
	548 *m*
	26 *m*
6	192 M
	17 M
	259 M
	433 M
	127 *m*
	175 *m*
	327 *m*
7	125 M
	156 M
	180 *m*
8	443 M
	282 M
	68 M
	435 *m*
9	316 *m*
	161 *m*
10	391 M
	403 M
	100 M
	141 *m*
12	292 *m*
15	345 M
16	9 M
	47 M
	137 M
	23 M
	77 *m*
	187 *m*
	171 *m*
17	418 M
	381 *m*
18	13 M
	220 M
	63 *m*
19	339 M
	277 M
	336 M
	114 M
	292 M
20	25 M
	55 *m*
	188 *m*
	171 M

July (*con.*).

d	No.
21	405 *m*
	44 *m*
22	85 M
	379 M
	296 M
	172 M
	95 *m*
23	105 M
24	62 M
	415 M
	400 M
	387 M
	436 *m*
	68 *m*
25	22 *m*
26	201 M
	150 *m*
	281 *m*
	11 *m*
	300 *m*
	361 *m*
27	65 *m*
28	327 M
	24 *m*
	396 *m*
29	33 M
	142 M
	50 *m*
	156 *m*
30	262 M
	253 M
31	21 M
	454 M
	195 M
	139 *m*

August.

d	No.
1	72 M
	109 *m*
	292 *m*
2	133 M
	449 M
	366 *m*
	380 *m*
3	440 M
	80 M
	288 *m*
4	148 M
	175 M
	460 *m*
	328 *m*
	125 *m*
	359 *m*
5	377 M
	438 *m*
6	418 M
	291 M
8	292 M

August (*con.*).

d	No.
8	332 *m*
	434 *m*
9	340 M
	236 M
	226 M
	138 M
	68 M
	306 *m*
10	37 M
11	204 M
	256 M
	156 M
	342 *m*
12	299 M
	104 M
14	364 M
	245 M
	334 M
	87 M
15	478 M
	358 M
16	382 M
17	125 M
	35 M
	422 *m*
	409 *m*
18	209 M
	374 M
19	109 M
	27 M
	380 M
	263 M
20	118 *m*
21	190 M
	327 *m*
22	322 M
	292 *m*
	466 *m*
24	171 *m*
25	302 *m*
	237 *m*
26	353 M
	418 M
	279 *m*
	68 *m*
27	456 M
	128 M
	351 M
	429 M
	31 M
	317 M
	157 M
	93 *m*
28	187 M
	333 M
	24 *m*
29	292 M
	171 M
30	252 *m*
	394 *m*

August (*con.*).

d	No.
31	359 M
	272 M

September.

d	No.
1	101 M
	223 *m*
	244 *m*
2	253 *m*
3	173 *m*
	156 *m*
4	355 *m*
5	148 *m*
7	314 M
	52 M
	57 *m*
8	303 M
9	50 M
	316 M
	337 *m*
10	219 M
11	121 M
	68 M
	292 *m*
12	227 M
13	328 M
	327 M
	79 M
14	112 M
	294 M
	437 *m*
	125 *m*
	216 *m*
15	36 M
	418 M
	242 *m*
16	156 M
	6 M
	109 *m*
	60 *m*
17	199 M
	349 M
	465 M
	175 *m*
	352 *m*
	32 *m*
18	451 M
	292 M
	369 *m*
	214 *m*
	433 *m*
19	411 M
20	186 *m*
21	258 M
	98 M
	161 M
22	117 *m*
24	269 M
	119 M
	4 M
25	286 M

September (*con.*).

d	No.
26	188 M
	29 *m*
27	125 M
	201 *m*
	389 *m*
	68 *m*
28	235 M
	176 M
	335 *m*
	346 *m*
	76 *m*
29	70 M
30	232 M
	287 M
	396 M

October.

d	No.
1	206 M
	454 *m*
	171 *m*
	292 *m*
2	153 M
	159 *m*
3	408 M
	73 M
	275 *m*
4	109 M
	3 M
	472 *m*
5	148 M
	418 M
	259 *m*
	467 *m*
	88 *m*
6	241 M
	185 *m*
	231 *m*
	327 *m*
7	171 M
	222 M
	438 M
8	292 M
	156 *m*
9	386 M
	458 M
	383 *m*
11	51 M
	278 *m*
	359 *m*
	380 *m*
12	436 M
	378 *m*
	354 *m*
13	74 M
	118 M
	254 M
	68 M
14	468 M
15	401 M

October (*con.*).

d	No.
15	151 M
	246 M
	177 *m*
16	432 M
	175 M
	94 M
17	39 *m*
	382 *m*
18	422 M
	110 M
	460 M
	63 M
	221 *m*
19	405 M
	108 M
20	243 M
	28 M
21	96 M
	156 M
	362 *m*
	49 *m*
22	452 M
	m
23	413 M
24	201 M
	41 M
25	418 M
	255 M
	427 M
26	455 M
	172 *m*
	125 *m*
	331 *m*
27	24 M
	474 M
	371 *m*
	48 *m*
28	380 M
	233 *m*
	21 *m*
29	450 M
	292 M
	239 M
	327 M
	50 *m*
	431 *m*
	68 *m*
30	34 M
	456 *m*
	47 *m*
31	187 M
	257 M

November.

d	No.
1	253 M
	328 *m*
	109 *m*
	54 *m*
	374 *m*

November (*con.*).

d	No.
1	192 *m*
	104 *m*
2	478 *m*
3	11 M
	90 M
	421 *m*
	33 *m*
4	5 *m*
5	462 M
	266 M
6	276 M
	359 M
	148 *m*
7	234 M
	87 *m*
8	125 M
	127 M
	425 M
	202 *m*
9	306 M
	434 M
	384 *m*
	171 *m*
10	140 M
	19 M
11	85 M
	44 M
	184 M
	281 M
	292 *m*
12	81 M
	173 M
13	237 M
	252 M
	409 M
	156 *m*
	25 *m*
14	418 M
	68 M
	9 *m*
	394 *m*
15	453 M
	43 M
	470 M
	171 M
16	366 M
17	346 M
	220 *m*
18	288 M
	292 M
19	109 M
	175 *m*
	391 *m*
	273 *m*
20	379 *m*
	62 *m*
21	342 M
	181 M
22	2 M
	97 M

November (*con.*).

d	No.
22	327 *m*
23	302 M
	457 *m*
24	248 M
25	283 M
	69 M
26	156 M
27	132 M
	464 M
	122 *m*
	447 *m*
29	381 M
	95 M
	167 *m*
30	442 M
	420 M

December.

d	No.
1	59 M
	265 M
	355 M
	56 *m*
	37 *m*
	68 *m*
	292 *m*
2	102 *m*
	303 *m*
3	89 *m*
	316 *m*
4	435 M
	418 M
	14 M
	253 *m*
5	223 M
	161 *m*
6	148 M
	121 M
	330 M
	125 *m*
7	55 M
	24 M
	180 M
	466 M
	191 *m*
8	208 M
	292 M
	149 *m*
9	267 *m*
10	50 M
	116 M
	215 M
	155 *m*
11	328 M
	376 M
	407 M
12	188 *m*
13	343 M
	480 *m*

December (*con.*).

d	No.
14	186 M
	327 M
	198 *m*
	396 *m*
	438 *m*
15	428 M
	238 M
16	340 *m*
	190 *m*
17	68 M
	473 M
	8 *m*
	109 *m*
	359 *m*
18	175 M
	166 M
	171 *m*
	156 *m*
	118 *m*
19	448 M
	210 M
	125 M
	205 M
	137 *m*
20	67 M
21	382 M
	277 *m*
	380 *m*
22	292 *m*
23	360 M
	145 M
	171 M
24	353 M
	418 M
	150 M
25	77 M
	332 M
	60 M
	203 M
26	437 M
	29 M
	201 *m*
27	93 M
28	421 M
	264 M
	300 M
	415 *m*
29	292 M
	154 M
	393 M
30	104 M
	422 *m*
	400 *m*
31	156 M
32	242 M
33	68 *m*
34	471 M
35	63 *m*

DOUBLE STARS.

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1900+.	Observer
		h m	° ′		°	″		
1.	Σ 3062	0 0·9	+57 52	6½ ; 7½	348·1	1·48	4·00	B.
2.	OΣ 12, λ Cassiop.	0 26·4	+53 58	6½ ; 6½	153·8	0·50	3·96	B.
3.	β 395	0 32·2	−25 19	6·5 ; 6·7	293·6	0·57	2·80	D.
4.	β 495	0 43·4	+18 9	8 ; 8	219·9	0·81	4·00	B.
5.	Σ 60, η Cassiop.	0 43·0	+57 17	3½ ; 7½	226·4	5·68	3·19	M.
6.	OΣ 20, 66 Piscium	0 49·2	+18 29	6 ; 6	315·0	0·43	3·54	W.B
7.	Σ 73, 36 Andromedæ	0 50·1	+23 2	6 ; 6½	20·1	1·05	3·90	W.B
8.	Σ 113, 42 Ceti	1 14·3	− 1 3	6½ ; 7	352·4	1·25	2·99	W.B
9.	*h* 2036	1 15·0	−16 19	7 ; 7½	11·3	1·15	3·93	B.
10.	*p* Eridani	1 35·7	−56 42	6 ; 6	223·0	8·12	1·05	T.
11.	OΣ 38, γ Androm. BC.	1 57·8	+41 51	5½ ; 6	113·8	0·45	3·81	Ls.
12.	Σ 205, γ Androm. AB.	1 57·8	+41 51	2½ ; 5½	61·8	10·20	3·81	Ls.
13.	Σ 228	2 7·3	+56 59	6½ ; 7½	90·8	0·65	3·99	B.
14.	Σ 305	2 41·8	+18 58	7½ ; 8	315·8	3·06	2·47	W.B
15.	Σ 333, ε Arietis	2 53·5	+20 56	5½ ; 6	206·8	1·13	2·06	Ls.
16.	β 525	2 53·5	+21 13	7½ ; 7½	129·0	0·28	3·33	Ls.
17.	Σ 367	3 8·9	+ 0 22	8 ; 8½	205·4	0·57	4·15	B.
18.	OΣ 53	3 11·3	+33 16	7 ; 8	228·4	0·64	3·20	Ls.
19.	Σ 412, 7 Tauri	3 28·5	+24 8	6½ ; 7	4·6	0·25	3·47	W.B
20.	Σ 425	3 33·8	+33 49	7½ ; 8	271·1	2·42	4·04	W.B
21.	OΣ 531	4 0·8	+37 49	6½ ; 8½	134·4	1·69	3·20	Ls.
22.	OΣ 82	4 17·1	+14 49	7 ; 9	110·2	0·65	4·18	B.
23.	β 883, AB	4 45·6	+10 54	7½ ; 8	90·9	0·24	3·05	Ls.
24.	„ AC	......	......	7½ ; 10	157·6	17·60	3·06	Ls.
25.	Hussey 445	4 55·8	+20 41	8½ ; 8½	290·4	0·47	4·21	Ls.
26.	Aldebaran	4 30·2	+16 18	1 ; 13½	109·3	31·04	0·42	Ls.
27.	OΣ 98, ι Orionis	5 2·4	+ 8 23	6 ; 6½	169·5	0·92	4·10	B.
28.	Σ 742, 380 Tauri	5 30·4	+21 56	7 ; 7½	259·7	3·52	3·29	B.
29.	Σ 749	5 30·9	+26 53	6½ ; 7½	169·4	0·73	3·21	B.
30.	Σ 932	6 28·5	+14 49	8 ; 8	327·2	1·94	4·05	B.
31.	OΣ 149	6 30·0	+27 22	6½ ; 9	275·9	0·64	3·17	Ls.
32.	Σ 948, 12 Lyncis AB	6 37·3	+59 33	6 ; 6½	117·2	1·51	2·22	M.
33.	Sirius	6 40·6	−16 34	1 ; 10	127·9	6·31	3·12	D.
34.	OΣ 156	6 41·6	+18 18	6½ ; 7	298·3	0·50	3·12	W.B.
35.	Σ 1037	7 6·6	+27 24	7 ; 7	300·4	0·75	4·15	B.
36.	OΣ 170	7 12·2	+ 9 29	7 ; 7½	107·0	1·18	3·23	Ls.
37.	Σ 1066, δ Geminor.	7 14·1	+22 10	3 ; 8	208·4	6·99	3·21	B.
38.	β 579	7 27·9	+33 20	8 ; 10½	215·6	1·04	4·26	B.
39.	Σ 1110, Castor	7 28·2	+32 7	2 ; 3	224·8	5·61	3·09	Dob.
40.	Procyon	7 34·0	+ 5 29	1 ; 8½	344·9	5·39	2·21	Ls.
41.	Σ 1187	8 3·2	+32 31	7 ; 8	222·9	2·13	3·27	B.
42.	Σ 1196, ζ Cancri AB	8 6·4	+17 59	5½ ; 7½	352·4	1·05	4·26	W.B
43.	„ „ AC	......	......	5½ ; 6½	110·2	5·43	4·26	W.B
44.	„ „ BC	......	......	7½ ; 6½	122·5	6·08	4·26	W.B
45.	Σ 1216	8 16·3	− 1 17	7½ ; 8	195·3	0·46	4·23	B.
46.	Hough 354	8 36·6	+26 4	8 ; 8½	182·8	1·00	4·26	Ls.
47.	Σ 1273, ε Hydræ AB	......	......	4 ; 6	147·0	0·23	4·24	Ls.
48.	Σ 1273, ε Hydræ AC	8 41·5	+ 6 50	4 ; 7	232·2	3·51	4·24	Ls.
49.	Σ 3121	9 11·9	+29 0	7½ ; 7½	25·5	0·75	3·25	B.
50.	OΣ 201	9 18·0	+28 20	7 ; 11	221·2	1·52	3·18	W.B
51.	Σ 1356, ω Leonis	9 23·1	+ 9 30	5½ ; 7	116·4	0·94	3·16	Dob.
52.	Σ 1389	9 45·3	+27 29	8 ; 9	311·7	2·13	3·39	B.
53.	A. C. 5, 8 Sextantis	9 47·5	− 7 37	5½ ; 6	85·1	0·49	4·17	B
54.	OΣ 215, P. x. 23	10 10·9	+18 15	6½ ; 7	207·5	0·92	3·20	Dob.
55.	Σ 1424, γ Leonis	10 14·4	+20 22	2 ; 4	116·9	3·75	3·16	Ls.
56.	OΣ 218	10 22·1	+ 4 5	7 ; 9	75·5	1·09	3·29	Ls.
57.	OΣ 224	10 34·5	+ 9 22	7 ; 9	300·7	0·59	3·27	Ls.
58.	OΣ 229	10 42·3	+41 38	6 ; 7	319·3	0·88	3·40	Dob.
59.	Σ 1523, ξ Ursæ Maj.	11 12·9	+32 6	4 ; 5	137·0	2·39	4·42	M.

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1900+.	Observer.
		h m	° ′		°	″		
60.	Σ 1536, ι Leonis	11 18·7	+11 5	4½ ; 7½	53·9	2·45	3·22	Dob.
61.	OΣ 235	11 26·7	+61 38	6 ; 7½	134·5	0·50	3·41	Dob.
62.	Lalande 21846	11 24·0	+31 1	7 ; 11	5·0	1·00	4·39	Ls.
63.	Σ 1647, 191 Virginis	12 25·5	+10 16	7½ ; 8	225·3	1·55	4·30	W.B.
64.	Σ 1670, γ Virginis	12 36·7	− 0 54	3 ; 3	328·3	5·89	3·22	Dob.
65.	Σ 1687, 35 Comæ	12 48·1	+21 48	5 ; 8	80·5	1·04	3·47	M.
66.	β 112	12 55·2	+19 1	9 ; 9½	293·8	1·92	2·32	Ls.
67.	OΣ 266	13 23·6	+16 14	7 ; 8	341·8	1·63	3·33	Ls.
68.	Hough 260	13 19·0	+29 45	8 ; 8½	326·3	0·86	4·36	Ls.
69.	Σ 1768, 25 Can. Ven.	13 33·2	+36 48	6 ; 8	133·4	1·11	4·42	W.B.
70.	Σ 1785	13 44·5	+27 29	7½ ; 8	289·2	1·45	2·57	M.
71.	β 801	13 40·7	+11 20	8 ; 10	326·5	2·63	4·46	W.B.
72.	Σ 1819	14 10·3	+ 3 36	7½ ; 7½	176·1	1·34	3·31	Dob.
73.	α Centauri	14 32·8	−60 25	1 ; 1	210·7	22·01	1·33	T.
74.	Σ 1865, ζ Boötis	14 36·3	+14 10	4 ; 4	151·6	0·42	4·23	B.
75.	Σ 1877, ε Boötis	14 40·6	+27 30	3 ; 6½	330·3	2·75	3·39	B.
76.	Hough 263	14 43·3	+24 30	7 ; 10	177·2	0·97	4·51	W.B.
77.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	181·1	2·48	4·55	M.
78.	β 31	14 47·8	+19 9	8½ ; 9½	193·5	1·50	4·50	W.B.
79.	OΣ 287	14 47·9	+45 20	7½ ; 7½	322·5	0·71	3·35	Le.
80.	Σ 1932, 1 Cor. Bor.	15 14·0	+27 13	6½ ; 7	156·8	0·70	3·71	B.
81.	Σ 1937, η Cor. Bor.	15 19·1	+30 40	6 ; 6	22·3	1·03	4·62	M.
82.	Σ 1938, μ^2 Boötis	15 20·7	+37 22	6½ ; 7½	68·1	1·04	4·62	M.
83.	OΣ 298	15 32·4	+40 8	7 ; 7½	185·5	1·17	3·35	Ls.
84.	Σ 1967, γ Cor. Bor.	15 38·5	+26 36	4 ; 7	116·2	0·74	3·47	Dob.
85.	OΣ 303	15 56·2	+13 33	7 ; 7½	149·4	0·81	3·39	Dob.
86.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	285·5	0·34	3·52	B.
87.	Σ 2032, σ Coronæ	16 11·1	+34 8	6 ; 6½	213·5	4·58	3·43	Dbb.
88.	Σ 2055. λ Ophiuchi	16 25·8	+ 2 12	4½ ; 5½	58·9	1·38	3·49	Dob.
89.	Σ 2084, ζ Herculis	16 37·5	+31 47	3 ; 6½	193·9	1·05	4·62	M.
90.	Σ 2114	16 56·5	+ 8 36	6½ ; 8	162·2	1·21	3·43	B.
91.	Σ 2140, α Herculis	17 10·1	+14 30	2½ ; 6	114·7	4·62	3·46	B.
92.	Hough 415	17 19·4	+25 51	8 ; 8½	328·1	1·48	4·49	W.B.
93.	Σ 2173, 221 Ophiuchi	17 25·5	− 0 59	6 ; 6	326·7	0·93	3·49	D.
94.	Σ 2262, τ Ophiuchi	17 57·6	− 8 11	5 ; 6	260·0	1·68	3·74	M.
95.	Σ 2272, 70 Ophiuchi	18 0·4	+ 2 32	4 ; 6	187·7	1·83	4·59	M.
96.	OΣ 358	18 31·3	+16 54	6½ ; 7	192·0	1·82	3·50	W.B.
97.	β 648	18 54·2	+23 39	6 ; 9	220·0	1·11	1·61	Ls.
98.	γ Coronæ Aust.	19 0·0	−37 12	5½ ; 5½	143·3	1·93	1·40	T.
99.	Σ 2525	19 22·9	+27 8	7½ ; 7½	311·5	0·56	4·62	W.B.
100.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	297·8	1·68	3·01	M.
101.	Σ 2695	20 27·7	+25 28	6 ; 8	76·4	1·05	4·54	W.B.
102.	β 151, β Delphini	20 32·7	+14 15	3½ ; 5½	47·5	0·29	4·88	Ls.
103.	Σ 2744	20 57·8	+ 1 9	6½ ; 7½	163·0	1·33	1·72	Ls.
104.	Σ 2799	21 23·9	+10 38	7 ; 7½	117·0	1·37	3·56	B.
105.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	126·0	2·22	3·86	B.
106.	Σ 2878	21 50·6	+29 3	9 ; 9½	354·6	3·47	4·87	Ls.
107.	β 172	22 19·0	− 5 21	6 ; 6	7·4	0·73	2·30	D.
108.	OΣ 483, 52 Pegasi	22 54·2	+11 12	6 ; 7½	224·7	1·05	2·90	Ls.
109.	β 79	23 12·5	− 2 4	8 ; 8½	87·0	0·97	2·92	Ls.
110.	β 80	23 13·2	+ 4 53	8 ; 8½	9·3	0·45	2·92	Ls.
111.	β 720	23 29·0	+30 46	6 ; 6	349·0	0·34	3·95	Ls.
112.	Σ 3050	23 54·6	+33 10	6 ; 6½	216·8	2·36	3·64	Dob.

In the column headed "Observer," the following abbreviations are used:—

Dob. Doberck.	M. Maw.	B. Biesbroeck.	D. Doolittle.
Ls. Lewis.	T. Tebbutt.	W.B. Bowyer.	

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1904 December 9.

Prof. H. H. Turner, D.Sc., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
E. T. Whittaker, M.A.

The Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker. Sixty presents have been received since our last Meeting. Of these the following call for special notice:—2nd volume of Dr. Hinde's 'Treatise on Volume 14 of the Italian National Edition of the Works of Galileo'; the Smithsonian Institution has sent us two books, one a comparison of the features of the Earth and the Moon by N. S. Shaler, the other contains articles by the late Henry Draper on the silvered glass reflector, and by Mr. G, W. Ritchey on the modern reflecting telescope. Mr. Percival Lowell has sent us copies of his spectrograms taken to determine the rotation of the planets, and we have received 20 more of the series of enlargements of Astrographic Chart Plates from the Royal Observatory, Greenwich.

The President. Besides these valuable presents, the Treasurer announced at the Council Meeting this afternoon that the Executors of the late Mr. McClean had informed him that a valuable bequest had been made to the Society, and for this the Council expressed their appreciation. I am sure the Fellows will associate themselves with an expression of gratitude to the members of Mr. McClean's family. The bequest is, I believe, a sum of £2000. I should also like to add that among the presents which Mr. Whittaker has announced there is one from himself which his modesty did not allow him to mention—a valuable book on Analytical Dynamics. I will ask the Meeting to return thanks to the donors. It is customary at these Meetings for the Fellows assembled to appoint the Auditors of the Treasurer's Accounts which are to be submitted at the Annual Meeting: so I will ask the Fellows present to elect three gentlemen to fill this office.

On the motion of *Dr. Lockyer*, seconded by *Mr. Inwards*, Mr. Thwaites, Mr. Shackleton, and Mr. W. W. Bryant were appointed Auditors.

The President. According to what was arranged at the end of last Meeting, I will call upon *Dr. Rambaut* to give us his paper on "The Pivot-Errors of the Radcliffe Transit-Circle." I am sorry the Fellows have not had the opportunity to read this before the Meeting, but, in spite of all efforts, it has been found impossible to get the 'Monthly Notices' published by this date, so Dr. Rambaut will find your minds as unbiassed as at the last Meeting.

Dr. Rambaut explained at some length, by the help of diagrams thrown on the screen, the apparatus used at the Radcliffe Observatory to determine the pivot-errors. This apparatus followed in great measure a plan proposed by M. Hamy, of the Paris Observatory, with some modifications. The principal feature is a pivoted lever, one end of which, terminating in a knife-edge, rests on the pivot, the other end carries a small horizontal mirror, face downwards, which rests almost in contact with the object-glass of a vertical collimating telescope. Looking through the collimator, diffraction rings are seen because of the close contact between the mirror and object; when the closeness of the contact is altered—as it might be by rotating the telescope whose pivot-errors are to be determined, if those pivots are irregular—the number of the diffraction rings is altered. Dr. Rambaut had modified M. Hamy's method by attaching a pin to the centre of the axis on which the knife-edge rested instead of on the upper part of the pivot; by this means the irregularity only of the part of the pivot resting on the bearings was detected, and not of the upper part on which the knife-edge rested. A table of the pivot-errors found with this apparatus was shown, and it was pointed out that these would very largely explain discordances already found between the Radcliffe and other catalogues.

Mr. W. W. Bryant asked a question relating to the effect of the pivot-errors on the determination of the level-error, to which Dr. Rambaut replied.

Mr. H. W. Chapman read his paper "On the Validity of Meteor-Radiants deduced from Three Tracks."

The President. This mathematical investigation of the probability of an astronomical conclusion being real is interesting. It appears that when three meteor-paths intersect in a point, within small limits, there is a great probability that the point is a radiant, the chance being only 1 in 400 the other way.

Mr. L. N. G. Filon. This is a paper whose merits will appear better when seen in print, for the analytical difficulties that

Mr. Chapman has had to overcome can scarcely be appreciated from description at a meeting. The problem was suggested by Mr. Hinks some years ago. I do not think Mr. Chapman stated that the radius of the small circle which the three tracks had to cut was assumed to be one degree—in this case the chance against the radiant being fictitious would be about 400 to 1. I am glad to take the opportunity of congratulating Mr. Chapman on the completion of this difficult piece of work.

Mr. Hinks. The problem was suggested by me, but that is a very different thing to working it out. I should like to add my congratulations.

The President. Mr. Whittaker and I have both had the pleasure of looking through the paper, and can bear testimony to what has been said, that it can scarcely be properly appreciated unless read in print. I will ask you to return your thanks to Mr. Chapman.

The President. We have had two papers sent us by *Mr. Franks,* which I will ask Mr. Wesley to tell us something about.

Mr. Wesley. One of these notes is called by Mr. Franks "Dark Nebulosities." He thinks there are not only bright nebulæ, but also dark ones. Photographs are shown of four lenticular nebulæ, such as those to which Lord Rosse specially called attention. They were taken with the 20-inch reflector by the late Dr. Roberts, and each shows a dark band cutting through the nebula longitudinally, the band being apparently darker than the sky. The nebulæ are probably spirals seen edgeways. It is suggested that their extreme edges consist of a cooler, and therefore light-absorbing gas, and that this cooler edge projected on the brighter background of the nebula produces the effect of the dark bands.

The other paper by Mr. Franks refers to the photograph which is shown. His attention was called to this detached nebula in Cygnus by the paper and photograph published in the 'Monthly Notices' by Dr. Max Wolf. Mr. Franks has now photographed it with the 20-inch reflector, which gives a picture on a larger scale, showing more detail; it is something like the trifid nebula in Sagittarius. The most interesting feature is the dark space surrounding it, and Mr. Franks suggests that this effect is produced by the nebula being viewed through an exterior shell of cooler gas. It is obvious that there is a space around it comparatively bare of stars.

The President. We are very much obliged to Mr. Wesley for bringing this paper before us. It is an old suggestion—these dark nebulæ—but Mr. Franks has chosen his examples with marked success. The photographs are all very interesting.

Dr. Rambaut. Can Mr. Wesley tell us whether any of these photographs have been duplicated? In some of them it is obvious that there are dark spaces blacker than the surrounding sky. It would be interesting to know if this is so on the duplicate.

Mr. E. Robinson. Might I ask if there is any theory as to why the dark part of the ring shows in the sky at all? It is really darker than the surrounding sky.

Mr. Wesley. I think Dr. Roberts photographed several of these nebulæ some years ago. Whether in all cases the dark band was really darker than the sky, which is not absolutely black, I cannot say. With regard to the other question as to how the dark circle surrounding the nebula can be seen, it is shown simply by a number of the stars around it being apparently blotted out.

Mr. Whittaker then read a paper by *Prof. E. W. Brown*, "On the Completion of the Main Problem in the New Lunar Theory." He said:—Prof. Brown has arrived at the completion of the first and chief part of the lunar theory at which he has been working for so many years; this part consists in finding expressions for the co-ordinates of the Moon so far as its motion depends on the attraction of the Sun and Earth considered as particles. It will be remembered that this theory is formed on a new principle, which, though suggested by Euler, was first practically developed by Dr. G. W. Hill in his celebrated memoirs "Researches in the Lunar Theory" and "On the Part of the Motion of the Lunar Perigee which is a Function of the Mean Motions of the Sun and Moon." Hill chose as his variables rectangular co-ordinates referred to moving axes, and by taking the equations of motion in a special form was able to obtain series which represent a new kind of "intermediary orbit," or first approximation to the Moon's path. With this orbit as starting-point, he gave in the second paper a method for calculating the principal part of the motion of the perigee. After this the work was taken up by Prof. Brown, who, with the assistance of a computer, has performed something like 13,000 multiplications of series, involving about 100,000 coefficients in the products. The expansion obtained is algebraic in four of the five parameters, and numerical in the fifth (the ratio of the mean motions). The results will be accurate to $0''\cdot01$. The lunar inequalities arising from the planetary perturbations have still to be calculated.

Mr. Dyson described a paper by *Mr. P. H. Cowell.* He said:—This paper by Mr. Cowell is called "The Coefficients of 145 Terms in the Moon's Longitude derived from Greenwich Meridian Observations 1750–1901." Mr. Cowell has analyzed the differences between the observed and tabular longitudes of the Moon from 1750 to the present time, using Airy's reduction for the first hundred years, and the differences from Hansen's tables for the last fifty years. The work of this analysis involves a great deal of heavy work, the first step being to make a provisional theory correcting the tables used by Hansen and Airy for known errors. The coefficients of all terms in the lunar theory which are larger than $0''\cdot1$ are derived directly from the observations and compared with their theoretical values. The results are, I think, very remarkable as showing the close accordance of observations with theory for most of these coefficients. The accidental error of this work is shown very clearly because in the analysis the observed coefficients of the cosine terms, which have no existence in theory, are formed as well as those of the sine terms, and in this way it is shown that the probable error of one of these coefficients is 0″·025. In the analyses there were some difficulties to which attention is drawn. The terms which Mr. Cowell calls the allied terms, that is terms which differ by the Moon's elongation, present difficulties owing to the fact that there are no observations at New Moon. In Mr. Cowell's previous papers attention is drawn to these terms and the method of treatment explained. Another difficulty arises when there are two terms of nearly equal period. Mr. Cowell's analysis also shows that there is no term of coefficient more than 0″·5 unknown to theory except those given in his paper last month. Further, this work determines the values of the constants required in the lunar theory.

The President. These are two remarkable papers. Until recently we had been under the impression that we had rather an imperfect lunar theory, and that Hansen's theory might possibly require considerable modification. Prof. Brown, having made a discussion from theory by a perfectly new method, finds that Hansen's terms are all justified. On the other hand, Mr. Cowell has found the discordance between theory and observation, when properly treated, disappears, so we shall be in possession some time of a practically correct lunar theory. This is a remarkable result to arrive at in a few months.

The Astronomer Royal. I should like very much to have had an opportunity of seeing Prof. Brown's values and of comparing them with those found by Mr. Cowell. As to Mr. Cowell's work, I must express my satisfaction that he has been able to practically complete this, his first part of the work, the discussion of longitude, in so short a time. It is little over a year since he took it in hand, and it is practically now completed. As regards the discussion of latitude, I gather from him—and it is also obvious—that it will be a much lighter piece of work. He thinks it will be represented by about only half the amount of work involved in the longitude. Therefore the most important part of the work Mr. Cowell has taken in hand is now completed.

Mr. Crommelin. I understand, from private conversation with Mr. Cowell, that one of the largest discordances he gets from Hansen's theory will probably be explained by Hansen's empirical correction of the Moon's latitude, which amounts to 1″. Hansen applied a similar empirical correction to the longitudes owing to his supposition that the Moon's centre of figure and centre of gravity do not coincide. I do not know whether Mr. Cowell's paper states explicitly that that is justified by the observations, or whether he shows that the centre of the figure and the centre of gravity coincide. I believe Prof. Newcomb, in his lunar researches, found that Hansen's empirical terms do not really exist, and it would be interesting to know whether Mr. Cowell confirms Prof. Newcomb or not.

Mr. Dyson. I cannot, without reference to the paper, answer that. I do not think the point is considered.

The President. We shall see that when we read the paper. I will invite you to join in congratulating Mr. Cowell on the result of his work, and also Prof. Brown, who is now at the end of twelve years of labour. I had the privilege during a visit to America of spending a day with him, and I saw the box in which the calculations are, and I saw the smile on the face of the man who felt he was at the end of his calculations, and it was a very refreshing sight. It was in my mind to have had a discussion if there had been time on Mr. Maunder's paper read at the last Meeting, but I do not think that is possible now. I hope, before the next Meeting, that Fellows will have had it in print and be able to discuss it all the better. I should like Mr. Shackleton to show us a rather pretty mechanical slide which he has devised to show the phenomena of a total eclipse.

Mr. Shackleton showed the slide. The Moon was seen gradually to encroach on the Sun's disk, and when the latter was quite eclipsed the prominences and corona burst out.

Prof. Rambaut read a paper by *Mr. Wickham.* The object of the paper is to draw attention to Mr. Stanley Williams's new star or variable. From observations made on four days in October, Mr. Wickham, of the Radcliffe Observatory, found the magnitude of the star was, within very small limits, 9·5 in October. Circumstances prevented him watching the star again until December 3, 5, 7, and 8, and he found the magnitude on those days varied within very narrow limits, and came out at 10·5, so that in those two months the brightness has fallen a whole magnitude. Other particulars are given in the paper, but it is unnecessary to mention these now.

The following papers were announced and partly read:—

W. S. Franks. "Dark Nebulosities."

W. S. Franks. "Detached Nebula in Cygnus."

J. E. Gore. "On the Relative Brightness of Binary Stars."

Prof. E. W. Brown. "On the Completion of the Main Problem in the new Lunar Theory."

Prof. E. W. Brvwn. "The Final Values of the Coefficients in the new Lunar Theory."

A. Stanley Williams. "On the Relative Efficiency of Different Methods of Determining Longitudes on Jupiter."

H. W. Chapman. "On the Validity of Meteor-Radiants deduced from Three Tracks."

Royal Observatory, Greenwich. "Observations of the Leonid Meteors of 1904 November."

Royal Observatory, Greenwich. "Mean Areas and Heliographic Latitudes of Sun-spots in the Year 1903."

P. H. Cowell. "The Coefficients of 145 Terms in the Moon's Longitude derived from Greenwich Meridian Observations, 1750–1901."

W. F. Denning. "Note on the Shower of Leonids in 1904."

F. A. Bellamy. "A Comparison of the A.G. Catalogue (1900·0) for Vienna (Ottakring) with the Radcliffe Third Catalogue (1890·0)."

W. Wickham. "On the Decline of the Visual Magnitude of the Variable 159, 1904 Pegasi, as observed at the Radcliffe Observatory, Oxford."

The following gentlemen were elected Fellows of the Society:—

Wm. Allan, M.A., B.Sc., 88 Leamington Terrace, Edinburgh.
E. C. Atkinson, M.A., Erwood, Beckenham, Kent.
Col. A. H. Bagnold, R.E., Warren Wood, Shooters Hill, S.E.
Rev. D. B. Marsh, D.Sc., Hamilton, Ontario, Canada.
Wm. Newbold, 7 Broadwater Down, Tunbridge Wells.

The following Candidate was proposed for election as a Fellow of the Society:—

W. E. Raymond, Observatory, Sydney, N. S. Wales (proposed by H. C. Russell).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

President: A. C. D. Crommelin, B.A., in the Chair.

Secretaries: J. A. Hardcastle.
J. G. Petrie.

The first Ordinary Meeting of the fifteenth Session of the British Astronomical Association was held at Sion College, Victoria Embankment, on Wednesday, November 30th.

Mr. Petrie read the Minutes of the last Ordinary Meeting, which were confirmed and signed.

Mr. Hardcastle read the list of presents received, and the thanks of the Meeting were accorded to the respective donors.

The President called attention to the photograph of Dr. Isaac Roberts (presented by Mrs. Roberts), which was handed round for inspection.

The name of one Candidate for Membership was read and passed for suspension, and the election by the Council of nine new Members was unanimously confirmed.

The President said that, before the ordinary business of the Meeting was proceeded with, he should like to thank the Association for the honour they had conferred upon him in electing him to the Presidency. He had never undertaken any position of this kind before, and he hoped they would excuse any shortcomings on his part.

Mr. C. T. Whitmell read a paper on "Eclipses of the Midnight Sun." He remarked that it was a great pleasure to him to be the first to read a paper before their new President, whose friendship he had enjoyed for many years, and who had been more helpful to him in his endeavours for astronomy than almost anybody else he had ever known.

The President said they were very much indebted to Mr. Whitmell for his lucid exposition of these very puzzling, topsy-turvy eclipses. It was necessary to consider them, rare as they were, in order to have a thorough collection of all possible types, and although they were rare, yet nearly every eclipse—not quite all, but most — went through this type, either at the beginning or at the end of their career as members of a cycle in the Saros.

Mr. Gavin J. Burns read a paper on "The Total Light of the Stars." He said that the total light received from all the stars had been differently estimated at amounts varying from $\frac{1}{10}$th to $\frac{1}{160}$th of the light of the full Moon. Mr. Gavin exposed about one-third of a quarter-plate at the bottom of a box having a circular hole at the top, the diameter of which was twice the depth of the box. The time of exposure was generally one hour. The plate was then put away till some suitable night when the Moon was full. A series of eight exposures was then made on the other part of the plate. These exposures varied from 3 to 20 seconds. The result was to make 4 seconds' exposure to moonlight equal to 1 hour's exposure to skylight, and since the effective light of the sky in the experiments was equal to $\frac{1}{8}$th of the whole sphere, it followed that the total light of all the stars was to the light of the full Moon as 8 to 900, or a little under 1 per cent.

The President asked where Mr. Burns's experiments were made.

Mr. Burns. At Blackheath.

The President said it was difficult in the neighbourhood of London to be sure that all the light they were getting was really starlight. Prof. Newcomb, he believed, made his observations 20 miles distant from any source of artificial light that would disturb the results. Everything possible should be done to encourage researches like these, and they were therefore much indebted to Mr. Burns for his paper.

Mr. W. T. Lynn read a paper on "Encke's Comet," giving a brief history of its appearances from its first discovery by Méchain at Paris in January 1786. He remarked that when he wrote the paper he had not seen Dr. Smart's letter pointing out that the comet approached very near Mercury at the present return. It was noteworthy that there was a commensurability between the periods of Mercury and of the comet, for 15 periods of the latter amounted to about 18,150 days, whilst 206 periods of the planet amounted to 18,128 days. Might not this near correspondence cause regular perturbations which would produce the effects noticed on the motion of the comet?

Mr. H. Ellis asked whether, as the number of times the comet and Mercury coincided were so few, that would account for a regular diminution in the comet's orbit, as seemed to be suggested.

The President said that was a question that could not be answered straight away, but it was difficult to imagine that Mercury could produce so great an effect, as it was so very near the Sun, its speed so very great, and its mass evidently so very small.

The President then mentioned that Mr. Chambers had collected a good deal of information about the conditions in Spain for viewing the coming eclipse.

Mr. Chambers read a paper embodying the information he had obtained, under the headings of (1) Probabilities of Climate, (2) Routes to Spain, (3) Methods of Travel, and concluding with a suggested scheme of itinerary covering about three weeks.

Mr. Maunder said that the Association were much indebted to Mr. Chambers for the thoroughness with which he had been enquiring into the various facilities for travelling to Spain on the occasion of the eclipse next year, and it was very desirable that those contemplating taking part in the observation of the eclipse should send in their names at once.

Mr. Keatley Moore. To whom?

Mr. Maunder replied that communications should be sent to himself as Secretary of the Eclipse Committee.

The President, in thanking Mr. Chambers and Mr. Maunder for the information they had laid before the Meeting, joined them in urging that names should be sent in earlier than was done in the case of the other eclipse expeditions, so that it might be possible to make favourable arrangements with the steamboat companies.

Mr. Maunder then gave the substance of a paper which he had recently read before the Royal Astronomical Society, a report of which has already appeared in these pages, on "The Solar Origin of Terrestrial Magnetic Disturbances."

Mr. Alex. J. S. Adams said that he had had the opportunity of reading a report of Mr. Maunder's paper, and had noted down a few points before he came to the Meeting. He thought that the paper constituted a valuable contribution to their knowledge of the phenomena of sun-spots and magnetic variations, but he should hesitate before accepting the idea of a solitary force-patch upon the Sun. The coronal streamers were certainly not straight lines in the sense required. He rather thought that the results were due to some form of solar tide. It had been suggested that the Earth's gravitation might affect the growth and decay of sunspots. This seemed to him hardly a tenable hypothesis, and yet the statistics showed that a preponderance of magnetic storms occurred either at full or new Moon.

Mr. McCarthy said that the idea of a coronal streamer some 90,000,000 of miles in length sweeping round in 27 days involved a velocity for that part of the streamer which touched the Earth that appeared to him quite incredible.

Mr. Alex. Hutt asked why the principal magnetic disturbances at Greenwich seemed always to occur about midnight.

Mr. Newbegin inquired whether there was any interval between the passage of a spot across the central meridian and the disturbance of the magnetic needle.

Mr. C. Thwaites wished to know whether there was any evidence as to the latitude of the disturbing regions. Were they distributed in certain zones north and south of the Sun's equator?

Miss Stevens asked whether the great magnetic storms had been recorded in other parts of the world—for instance, at the observatories of Melbourne and Sydney.

Mr. Maunder, replying to the last speaker, said he had not yet been able to examine any records, except those of Greenwich, but he hoped to be able to do so at an early date. In reply to Mr. Thwaites, strictly speaking the only indication which the times of the magnetic disturbances gave was that of longitude of the centre of the Sun's disk, but the varying rotation-periods of the different latitudes 0° to 30° were indicated. The rotation-period of the polar regions would not correspond at all. In reply to a further question, Mr. Maunder said that assuming that the force started when the spot was at the centre of the disk, it took, on an average, about 26 hours to reach us. With regard to the motion of the coronal streamers there was really no difficulty. They must not be regarded as solid, but just exactly like the tail of a comet, which would point in one direction from the Sun before perihelion and a very few hours afterwads in the opposite direction.

The President said that there was no doubt that Mr. Maunder's paper was a notable contribution to our knowlege of the Sun and of the Earth's magnetic disturbances. The results about the 27-day period stood quite apart from any speculations in connection with the corona, and indicated that there were these more or less permanent centres of force on the Sun, and would tend to throw a great deal of light on our ideas and knowledge of the Sun's interior.

ROYAL METEOROLOGICAL SOCIETY.

At the Meeting of this Society on the evening of Wednesday, December 21, held at the Institution of Civil Engineers, *Capt. D. Wilson-Barker, F.R.S.E.*, President, being in the Chair, an interesting discussion took place on the subject of "Fog." This arose out of a paper by *Mr. F. J. Brodie* on the "Decrease of Fog in London during recent Years," which was read at the previous Meeting.

A paper by *Dr. W. N. Shaw, F.R.S.*, and *Mr. W. H. Dines,* on "The Study of the Minor Fluctuations of Atmospheric Pressure," was also read. The authors described an apparatus called the "Micro-Barograph," which they have designed to magnify the minor fluctuations, and at the same time to disentangle them from the general barometric surges. They also showed some interesting records from three of these instruments. The authors wish to obtain information as to the nature of the disturbances and the causes to which they may be assigned. Among the causes which suggest themselves as likely to produce temporary fluctuations of the barometric curves are stated by the authors to be:—(1) Atmospheric billows passing along surfaces where there is discontinuity of density in a manner somewhat similar to ocean waves; (2) the passage of minute whirls, or cyclonic depressions of small scale; (3) variations of pressure due to the attraction or repulsion produced by electric stress as masses of air at different potential pass over; (4) the mechanical effects of wind; and (5) the mechanical effects of rapid condensation of aqueous vapour.

Fireballs in January.

With the exception of the well-known shower of Quadrantids or Boötids which marks the opening of the year, January has been usually regarded as possessing little interest for the meteoric observer. In England the nights are generally overcast, and when they are clear only a few meteors are presented in the frosty air.

But during the past few years January has furnished us with a number of very large fireballs, a few of which were sufficiently brilliant to be visible in the daytime, as, for example, those of 1901 January 6 and 1900 January 9. The various fireballs observed apparently favour no particular radiant, and are distributed over many nights, though several distinct periods of prevalence appear to be indicated. It will be advisable to watch for fireballs during the ensuing month and to carefully note the apparent paths of any that may be visible.

During the five years 1900–4 there were 31 fireballs on the following dates:—

Jan. 6	1	Jan. 22	1
7	1	23	1
8	1	24	4
9	2	25	1
10	4	26	1
13	3	28	4
14	1	29	1
15	1	30	1
18	1	31	1
19	1		

Thus two periods seem well defined, viz. January 9–13 and January 24–28, and it is hoped that not only meteoric observers but astronomers generally will keep an eye on the firmament at these special times. There may be several active showers which have had a share in contributing the fireballs of recent years. Past observations are neither sufficiently exact or numerous to enable us to judge with certainty; in fact the great majority of large meteors seen have been described so indefinitely, by those who were fortunate enough to witness them, that nothing reliable can be deduced as to their real paths in the air.

These abundant, and often exceedingly brilliant, January fireballs are interesting as occurring in the depth of the winter season and as affording evidence of radiants not hitherto well determined. During the ensuing few years it may be possible to accumulate much new data as to the visible aspects and flights of these magnificent visitors and more nearly perfect our knowledge concerning them.

Bristol, 1904, Dec. 7. W. F. DENNING.

On a Cause of Variability of Division-Errors in certain Graduated Circles *.

IN instruments of precision the division of circles is generally made on silver; this metal can be cut finely by the tracer of the dividing-machine, and thus yields lines sufficiently unalterable which can be bisected with accuracy.

On account of the high price of this metal, the circle cannot be made entirely in silver, so another cheaper metal is employed, used as a support, and in this is inlaid a thin plate of silver on which is traced the divisions.

For the metal support brass or bronze has been employed, and it is still used to-day for circles of small dimensions; but for half a century cast-iron has often been used for large astronomical instruments, in spite of the want of homogeneity of the substance.

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* By M. G. Bigourdan. *Comptes Rendus*, 1904, Sept. 26. (Translation.)

Divided circles all have division-errors, which are, however, carefully determined in the instruments of the observatory; generally the operations are multiplied so that, as shown by the accordance of results, the error of each division may be known to about 0″·1 at least, and it is thought that afterwards the errors remain invariably the same.

If, however, after an interval of some years, one determines anew the errors of the same cuts, results are found which, perhaps, differ from the first by quantities relatively large, or the hypothesis of the permanence of division-errors is not verified by experience; and I propose to show that this variation of division-errors is to be explained simply by the influence of variations of temperature, even independently of the non-homogeneity of the support.

Let us consider, then, a circle formed from a solid mass of cast-iron, on the periphery of which there is inlaid a silver strip. This strip, thin and plastic, is evidently obliged to obey the dilatations of the cast-iron when the temperature varies.

The expansions of cast-iron and silver, expressed in microns are respectively 20 μ and 11 μ per metre per degree centigrade, that is a difference of 9 μ. As to the interval of extreme temperatures of the year, in our climate it generally exceeds 40°. In a circle of average size of one metre diameter, for example, the difference of dilatation of the silver strip, compared with the cast-iron, reaches then, in the course of the year, $9\,\mu \times 3{\cdot}1415 \times 40$, or 1·13 mm. Such is the expansive force that the silver undergoes as we pass from summer to winter, and the contractile force is of the same amount as we pass from winter to summer.

To have division-errors quite permanent, it is necessary that, under the influence of these deformations, each point of the silver strip should be displaced only in the direction of the corresponding radius; on the other hand, this geometrical condition can evidently only be fulfilled in rare cases, because the substances which react one on the other are not perfectly homogeneous, and because the fixing of the silver strip in the cast-iron cannot be done everywhere in the same manner.

In the circle supposed of one metre diameter, an angle of 1″ corresponds to an arc of 2·43 μ in length; to the degree of approximation supposed ($\pm 0''{\cdot}1$) the division-errors would then remain invariable only if the lateral displacements were less than 3·24 μ, a quantity sensibly equal to $\frac{1}{5000}$ of the total change of length.

With brass or bronze as the metal support, these difficulties are almost entirely eliminated, because the dilatation of these alloys is a little less than that of silver.

We ought, then, to reject the use of cast-iron as a support in divided circles, and should prefer brass or bronze. But it is much better still to make the circle of a single metal, which should be not very costly, not changeable, should be capable of polish, and one that can be cut without tearing by the cutter of the dividing-engine. Nickel and certain nickel-steels appear to fulfil all these conditions *.

Real Paths of Leonids.

THE brightest meteor seen at Greenwich (according to the list kindly supplied at my request by the Astronomer Royal) during the recent Leonid display appeared on November 16^d 16^h 24^m 42^s. It was observed by Messrs. Showell, Parkinson, and Barrett, and its apparent brilliancy much exceeded that of Jupiter. Near the termination of its flight it burst and lit up the entire sky. The path extended over 40°, traversed in 3 seconds, and the direction was from between Castor and Pollux towards the Pleiades. The streak was visible for 1^m 54^s.

The meteor was recorded by Mr. Charles L. Brook at Meltham, near Huddersfield, and he describes it as much brighter than 1st mag. It gave a green flash and left a short streak amongst the stars low in the S.S.E. sky for 1 minute. Observed path from 130° −9° to 120° −23½°; duration of flight 1 second.

The same meteor was recorded by Mrs. R. M. Brook at Charmouth, Dorset. Path 188° +42½° to 215° +48°. Motion rather swift; a streak remained visible for some time.

Comparing the three observations, the radiant-point is indicated at 151° +22°. The height of the meteor was from 88 to 44 miles from near Petersfield to Hungerford. The length of course seems to have been variously estimated at the several stations, but it was probably not more than 60 miles, and the velocity about 46 miles per second. The observers at Greenwich apparently traced the meteor through a more extended arc than that recorded elsewhere, but with the altitude of the radiant more than 50°, Leonids (even the brighter specimens) are rarely more than 60 miles long.

Another pretty conspicuous member of the same shower was observed on November 14 10^h 26^m by Mr. Parkinson at Greenwich, and by the Rev. W. F. A. Ellison at Enniscorthy, Ireland. This accordance is very noteworthy in regard to two features, namely, the wide distance (about 280 miles) separating the two stations and the low position of the Leonid radiant. It had only just risen at Greenwich, while at Enniscorthy it was below the horizon.

The descriptions of the observed path are not very definite as to the places of beginning and ending, but the meteor was evidently a Leonid with a long horizontal flight from over the neighbourhood of Sheffield to near Caermarthen. The height was from about 83 to 78 miles, and approximate velocity 40 miles per second.

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* Silver is not without defects as a material for the construction of divided circles, for it blackens quite rapidly, especially in large towns, on account of hydrosulphuric emanations. Even if one took the greatest precautions the removal of the black dirt which is thus produced must change the look of the lines, and consequently alter the way in which they are bisected, which in turn would alter the division-errors.

A Leonid of 2nd mag. was observed on November 14 $14^h\ 37^m$ at Bristol and Charmouth. It fell from a height of 79 to 58 miles from over Faringdon to Stroud; visible path 35 miles, and velocity 39 miles per second.

Several other meteors appear to have been doubly observed during the recent display, but there are comparatively few such agreements, though a large number of observations have been sent in. Indeed one cannot help wondering at the rareness of these duplicate records. In cases where the stations are a moderate distance apart and watches are maintained at same hours, we should expect to find that a relatively large number of identical meteors had been seen at both places, but this is very far from being realized when the individual paths are carefully compared.

Even at times when quite an abundant display of Leonids is in progress, the brightest meteor of all frequently proves to be a Taurid or Arietid. This happened in 1903, and has been repeated in 1904, for there was a magnificent fireball seen at Charmouth, Torquay, Dublin, Paris, &c., on November 15 at $14^h\ 39\frac{1}{2}^m$. It sailed across the southern sky (as seen from the S. of England) in a direction from W. to E., and quite contrary to that of the swarm of Leonids, its radiant-point being probably near α or ϵ Arietis. But a sufficient number of accurate observations have not yet been received to enable the real path to be very precisely determined.

Bristol, 1904, Dec. 17. W. F. Denning.

The Satellite of Sirius.

The fact that the faint companion of Sirius has a mass equal to that of the Sun is so anomalous that it seems to deserve more consideration than it has hitherto received. According to the orbit recently computed by Zweirs (Period = 51·101 years and $a = 7''\cdot77$) and the parallax of $0''\cdot37$ found by Sir David Gill, I find that the mass of the Sirian system is 3·5465 times the Sun's mass; and as, according to Auwers, the masses of the bright star and its faint attendant are in the ratio of 2·119 to 1, we have the mass of the satellite equal to 1·137 × Sun's mass. For simplicity of calculation let us assume that the mass is equal to the Sun's mass. With the above parallax I find that the Sun, if removed to the distance of Sirius, would be reduced to a star of 2·23 magnitude. The magnitude of the Sirian satellite is usually assumed to be 10, but let us assume that it is $7\frac{1}{2}$ magnitudes fainter than the Sun would be in the same position. This would make its magnitude

$$2\cdot23 + 7\cdot5 = 9\cdot73,$$

which is near enough to the truth for our present purpose. A difference of $7\frac{1}{2}$ magnitudes implies that the Sun is 1000 times brighter than the satellite. Now the faintness of this small star must be due either to its small size or to its small luminosity of surface per unit of area, and I proceed to show that the latter supposition is the only tenable hypothesis (I have shown elsewhere that it could not possibly shine merely by reflected light from Sirius).

If its faintness were merely due to its small size, that is small volume, its surface luminosity being equal to that of the Sun, the Sun's diameter should be $\sqrt{1000} = 31·6$ times the diameter of the faint star in order to produce the observed difference of light. But on this hypothesis the Sun would have a volume $= 1000^{\frac{3}{2}}$, or 31,630 times the volume of the star, and as the density of a body is inversely proportional to its volume, we should have the density of the star equal to $31,630 \times 1·4 = 44,282$ times that of water (Sun's density $= 1·4$). This is, of course, entirely out of the question, and shows that the luminosity of the satellite's surface cannot possibly be comparable with that of the Sun.

Let us now consider what luminosity of surface it could have consistently with a comparatively low density. As it shines by inherent light it must be comparatively hot and hence its density cannot be high. If we assume a surface luminosity of say 0·01 of that of the Sun—or about that of the binary companion of 40 (o_2) Eridani — we have, surface of Sun $= 10$ times surface of satellite. The Sun's volume would then be $10^{\frac{3}{2}}$, or 31·63 times that of the star, and the density of the satellite in this case would be

$$31·63 \times 1·4 = 44·28$$

(water $= 1$). But this is also quite inadmissible, as there is no known substance with nearly so high a density.

Assuming a luminosity equal to 0·002 of the Sun's luminosity, we have,

$$\text{surface of Sun} = 1000 \times 0·002 = 2·0 \times \text{surface of satellite.}$$

This would make the

$$\text{density of the satellite} = 2^{\frac{3}{2}} \times 1·4 = 3·96,$$

which seems an improbably high density for a self-luminous body.

If we assume a luminosity of 0·0015, we have

$$\text{density of satellite} = (1·5)^{\frac{3}{2}} \times 1·4 = 2·57,$$

which is, perhaps, a possible value.

A luminosity of 0·001 would of course make it of the same volume and density as the Sun. But if it had the same density as the Sun there would seem to be no reason why its luminosity of surface should be so small.

We may therefore conclude, I think with great probability, that the luminosity of surface of the Sirian satellite does not exceed 0·0015 that of the Sun. But even this small luminosity would give a considerable amount of light. Assuming that sunlight is 600,000 times moonlight, we have the luminosity of the satellite equal to 900 times that of the Moon's surface. It is, however, extraordinarily small for a body shining by inherent light, and the satellite must be far advanced on the road to the total extinction of its light. It is unfortunate that its spectrum cannot be observed. It should be a most interesting one. But perhaps this may be possible in the future with a gigantic reflecting telescope.

We have a very similar case in the satellite of Procyon. If its magnitude has been correctly estimated at 13, we have a difference of over 10 magnitudes between the Sun and the satellite, and its luminosity would be even less than that of the Sirian satellite. Its mass is, however, smaller, about 0·60 that of the Sun.

It is, of course, possible that the faint satellite of Sirius may be a very large globe of very low density and small luminosity, like the gaseous nebulæ. In that case we should expect to find a spectrum of the nebular type, that is one with bright lines on a dark ground. But its reddish colour points rather, I think, to a cooled-down sun, in which case it would probably show a banded spectrum of the third type. From these considerations it seems that it must have either a comparatively high density or else a very low one, and its spectrum would decide the question.

In the case of the companion to Procyon, which seems to be of a "purplish colour," Mr. Lewis noted, in March and April 1898, that "the appearance of the companion of *Procyon* was not so like a star as that of *Sirius*, and that while the wire of the micrometer totally eclipsed the companion of *Sirius*, the companion of *Procyon* was seen on both sides of the wire" ('Monthly Notices R. A. S.,' April 1898). This is very suggestive and seems to show that this faint satellite is probably a small gaseous nebula.

Dublin, 1904, Dec. 17. J. ELLARD GORE.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Francisco Bianchini.

GENTLEMEN,—

This astronomer's name is probably best known for his attempt to determine the duration of the rotation of Venus, which he considered to be more than twenty-four days in length, instead of less than twenty-four hours, as J. D. Cassini had done, though the younger Cassini afterwards endeavoured to reconcile Bianchini's observations with his father's result. The admitted difficulty of the problem and the uncertainty whether any markings whatever are perceptible on the body of the planet, are such that neither of these durations (though Schröter's observations of an apparent periodic blunting of the horns afforded some confirmation to Cassini's) has secured acceptance, whilst Schiaparelli's, much longer than either, has not been substantiated, and spectroscopical observations of the limbs have hitherto only pointed to the probability of a somewhat rapid rotation.

My present object, however, is rather to call attention to other observations and labours of Bianchini *. He was born at Verona in 1662, and became librarian to Cardinal Ottoboni, who was afterwards Pope Alexander VIII. Most of his observations were made at Albano, the supposed site of the Abba Longa of the Romans, destroyed in the days of the kingdom by Tullus Hostilius. A careful selection of these observations was published folio at Verona in 1737 (eight years after his death), edited, with a preface, by Eustachio Manfredi, of Bologna, and a portrait of Bianchini forms a frontispiece. The volume gives a high idea of the skill, assiduity, and sagacity of the astronomer. He discovered three comets; of one of these he was not only the discoverer (on the 30th of June, 1684), but the sole observer; the second † seems to have been discovered almost simultaneously by him and by La Hire at Paris on the 20th of April, 1702; the third was discovered by him on the 17th of October, 1723, but it had been seen as early as the 1st of that month by Saunderson ‡ at Bombay, and others saw it before Bianchini. All these comets move in parabolic orbits; of the first he obtained a series of obervations of its course from the constellation Virgo through Boötes to the neighbourhood of Corona Borealis, where it was last seen on the 19th of July; the results were published in the 'Acta Eruditorum' for 1685, and the orbit was one of those determined by Halley. Bianchini made an attempt to determine the parallax and distance of Mars at its opposition in 1685, his result being that it was then about 5250 semidiameters of the Earth distant from us, which is not quite two-thirds of the true value. He observed a large number of eclipses of the Moon, phenomena of transits, occultations, &c. of Jupiter's satellites, and other phenomena; also delineated a considerable number of formations on the lunar surface, which will be found in Manfredi's selection. Another very interesting observation by Bianchini is of the solar eclipse of the 22nd of May, 1724, the central line of which passed over England, and was the last which was total in this country. His observations to determine the duration of the rotation of Venus were published at Rome in 1728 under the title 'Hesperi et Phosphori nova phænomena, sive Observationes circa Planetam

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* Prof. Newcomb, in his recent admirable work 'Astronomy for Everybody,' spells the name Blanchini. The Latin form, however, is Blanchinus.

† It was the second comet of the year, but the first was not sufficiently observed for determination of orbit.

‡ Carl ('Reportorium der Cometen-Astronomie,' p. 96) erroneously gives the name as Sauderson, and (following Pingré) overlooks some of his dates; the places are very rough.

Veneris.' The last observations recorded of Bianchini were of the eclipse of the Moon on the 13th of February, 1729. On the 2nd of the following March he died suddenly of the bursting of a blood-vessel, very nearly two years after the death of Newton.

Yours faithfully,

Blackheath, 1904, Nov. 7. W. T. Lynn.

Periodical Comets due in 1905.

Gentlemen,—

Let us hope that our hitherto constant friend, Encke's Comet, is not about to forsake us, but it has been particularly inconspicuous at this return, which may possibly be partly due to wastage resulting from old age. The present is the thirty-sixth return to perihelion since it was first seen in 1786, and the twenty-sixth since its period was determined by Encke in 1819. So poor has 1904 been in comets that it reckons as 1904 *b*, but, as it will not pass its perihelion until about the 4th of next month, it will probably be 1905 I. in a permanent list. Mr. Smart has called attention to the fact that in the second week in January it will approach Mercury within four millions of miles, but, in view of its faintness at this return, we shall probably have to wait for some time before being able to trace the effects of the planet's attraction upon the motion of the comet.

The only other known periodical comet due to return in 1905 is that which was first discovered by Prof. Max Wolf at Heidelberg on the 17th of September, 1884, and independently detected by Dr. Copeland at Dun Echt on the morning of the 23rd. The perihelion passage did not take place until the 18th of November, and the period was determined by Prof. Krüger and by Dr. Zelbr, the former making it 2391, and the latter 2470 days, or about 6·76 years. It reckoned as Comet III. 1884. At the return in 1891 it was first seen by Dr. Spitaler, at the Imperial Observatory, Vienna, on the morning of the 2nd of May, and two days afterwards by Prof. Barnard at the Lick. It passed its perihelion on the 3rd of September, and reckons at that return as Comet II. 1891. The first person to observe it at the next return, in 1898, was Mr. Hussey, who saw it at the Lick Observatory on the night of the 16th of June. The perihelion passage took place on the 4th of July, but the comet did not make its nearest approach to the Earth until about the end of November. At that return it reckons as Comet IV. 1898. The next will be due early next year.

Yours faithfully,

Blackheath, 1904, Dec. 7. W. T. Lynn.

Fine Lunar Rainbow.

Gentlemen,—

I have not seen any account of a lunar rainbow on the evening of September 26th, that lasted much longer than any I have witnessed. It came into sight here during a thunderstorm at 7.30, the Moon, three days past the full, barely shining in the opposite part of the heavens. It spanned an arc of about 60°, the greatest altitude being about 25°. At 7.35 it became much brighter, and the sky became lighter at the horizon, and Arcturus shone out within the rainbow. The eastern portion faded first, and the whole had vanished at 7.53.

Faithfully yours,
S. J. JOHNSON.

Melplash Vicarage, Bridport,
1904, Dec. 20.

Lunar Angles.

GENTLEMEN,—

On p. 457 of the *Observatory* for December 1904 reference is made to an error in connection with the inclination of the Moon's axis to the plane of the ecliptic.

It is not a little curious that there is, so far as I am aware, no book, either on general or on lunar astronomy, in which the following angles are clearly and correctly tabulated in connection with each other:—

Inclination of
(1) Moon's orbit to ecliptic, 5° 9′.
(2) Moon's equator to ecliptic, 1° 31′.
(3) Moon's equator to Moon's orbit, 6° 40′.
(4) Moon's orbit to Earth's equator, $\omega \pm 5° 9'$.
(5) Moon's equator to Earth's equator, $\omega \mp 1° 31'$.

Here ω is the obliquity of the ecliptic, and (4) and (5) indicate the limits of the variation.

The values above given are only approximate.

In Neison's work (p. 12) he refers to (2) as the inclination of the Moon's equator to her orbit, instead of to the ecliptic.

Proctor (' The Moon,' p. 393) gives (2) as the inclination of the Moon's axis simply. To make this correct, the words "to a perpendicular to the ecliptic" should be added. To a note on p. 174 a similar correction is needed.

Mr. Chambers (' Handbook,' vol. i. p. 119) gives (2) as the inclination of the Moon's axis to a perpendicular to the Moon's *orbit.*

I send this brief note in the hope that it may perhaps clear up a subject not very lucidly dealt with in the books which I have consulted.

Yours faithfully,
C. T. WHITMELL.

Leeds, 1904, Dec. 19.

[With reference to this subject, this may be a convenient place to say that Mr. S. A. Saunder has pointed out to us that the quotation of Hartwig's value of the inclination of the Moon's axis to the ecliptic was a little misleading, because it has been shown by Franz that if Hartwig's reductions had been carried to a second approximation, Hartwig's observations would have given 1° 31′ 27″, closely agreeing with Franz's result. A value of this quantity recently found by Hayn is 1° 32′ 16″ (*A. N.* 3956).—EDS.]

On a remarkable Error in Damoiseau's Tables of Jupiter's Satellites and in the 'Mécanique Céleste.'

Gentlemen,—

On comparing the values of the nodes of the orbits of the satellites on their respective fixed planes, as given by different authorities, I came across a very curious circumstance, which may, perhaps, be of interest to your readers. The angles dealt with are those which Souillart calls $bt + \gamma$, *i. e.* the *descending* nodes. Laplace and Damoiseau give the *ascending* nodes, which both denote by Λ.

If we compare the values for 1750·0 as given in the 'Mécanique Céleste' (which depend on Delambre's discussion of eclipses) and in Damoiseau's introduction to his Tables, we find the following (the values from the 'Mécanique Céleste' have been converted from centesimal into ordinary degrees, and 180° has been added in both cases, as explained above):—

		Méc. Cél.	Damoiseau.
Satellite II.:	$b_1t+\gamma_1$	93°	282°
„ III.:	$b_2t+\gamma_2$	7	353
„ IV.:	$b_3t+\gamma_3$	255	98

There is thus a difference of 180° between the two authorities in two out of three cases. Apart from this the agreement is as good as can be expected with regard to the very small weight of a determination of the nodes from eclipses.

This error of 180° certainly seems very strange at first sight. It is, however, very easily seen how it arose. The elements were in both cases derived *from eclipses.* In this derivation the node is determined as the position of the longest eclipse, but there is no criterion whatever by which the descending and ascending nodes can be distinguished from each other. This can only be done by observations of *latitude.* Such observations were made by Sir David Gill in 1891 and discussed by me. They were compared with Souillart's theory, which is based entirely on Damoiseau's numerical data. The corrections to the theory were found to be small in all cases. This seems to show that Damoiseau's values are correct, and those of the 'Mecanique Céleste' are 180° in error. This conclusion is correct in the case of Satellite IV. For Satellite II., however, it is *Damoiseau* who is 180° wrong, but at the same time his (and therefore Souillart's) secular motion of the node is erroneous to such an extent that the original error of 180° happens to be almost exactly cancelled at the epoch of Sir David's observations. The motion of the node could not be derived from Sir David Gill's observations, or from any observations covering only a short period of time. The existence of this large error in Damoiseau's value (which is due to the use of an erroneous value for the compression of Jupiter) has been proved by Mr. Cookson from a comparison of his own observations, made in 1902 and 1903, with the Cape Observations of 1891. It is, moreover, entirely corroborated by the evidence from the fifth Satellite, and its reality cannot possibly be doubted.

The following table shows the values found by different observers for the angle $b_1t+\gamma_1$:—

		°
1750·0	Méc. Cél. (Delambre)	93
	Damoiseau	282
1891·75	Gill, de Sitter	25·4
1902·1	Cookson	273·1

Damoiseau's adopted yearly motion is

$$b_1 = -12^{\circ}{\cdot}05.$$

The value derived by Mr. Cookson is $-10^{\circ}{\cdot}86$. Bringing Damoiseau's value for 1750 up with the above motion we find, for 1891·75, 14°, differing only 11° from the value resulting from the observations. If, however, we use the correct (Mr. Cookson's) annual motion, we find 183^{c}. If we assume the value resulting from Sir David Gill's observations (the probable error of which is $\pm 0^{\circ}{\cdot}4$) to be the true node in 1891·75, and Mr. Cookson's value of b_1 to be the true motion, we then find, for the node, in 1750, 124°. Thus Damoiseau's value, corrected by 180°, is still 22° in error. This does not seem to me to be an extraordinarily large error, bearing in mind the weakness of the determination of the node from eclipses. The error of the 'Mecanique Céleste' is even larger. It may be remarked that also in the case of Satellites III. and IV., Damoiseau's values are nearer to the truth than those of Laplace (corrected by 180° when necessary). Making the same assumptions as above, we find that in 1836 the node as derived from Damoiseau's data differed 55° from the truth. Thus if Bessel had observed position-angles of Satellite II., as well as of III. and IV., Damoiseau's mistake would probably have been found out six years after the publication of his Tables instead of sixty-eight.

Yours faithfully,
W. DE SITTER.

Groningen, 1904, Dec. 23.

OBSERVATORIES.

UNITED STATES NAVAL OBSERVATORY, WASHINGTON.—Rear-Admiral Chester's "Report for the Fiscal Year ending June 30, 1904," has just come to hand. This report deals in a most complete manner with the work, the staff, and the needs of the Observatory. An interesting feature right at the beginning of the Report is the tribute paid to the late Watchman. Mr. Henderson, an old soldier, had been connected with the Observatory since 1868, and his faithful records in the Meteorological Journal of the Observatory rendered it one of the most complete in the Government service.

The 26-inch and 12-inch equatorials are under the charge of Prof. Skinner. With the 26-inch 174 observations of minor planets were made, also a valuable set of 337 inter-measures of the Satellites of Saturn, besides a number of observations of comets. Prof. Skinner also made 165 measures of double stars. The 12-inch was also employed in minor-planet work, as many as 259 measures being secured, besides numerous observations of comets and miscellaneous objects.

The 6-inch and 9-inch transit-circles were not in constant use, but over 12,000 observations were made. The state of the reductions is:—

Astronomische Zone Catalogue (−13° 50′ to −18° 10′).—The precessions and secular variations for 1900·0 are finished. The 1431 stars in Bessel's zones that are also in this catalogue have been reduced to 1900 for comparison, and those in Lalande, Argelander, and Munich I. and II. are being similarly reduced.

Washington Zone Catalogue.—The work of revising the zone-observations of 1846–49 has been pushed, the comparison of the mural zones with the Cordoba General Catalogue being completed, and zone-corrections have been deduced for about half the zones, and applied to the first 25 zones. The preliminary revision of the transit-zones is about two-thirds completed, and about one-fourth of the zones have been compared with the Cordoba Gen. Cat.

PUBLICATIONS.

THE THEORY OF OPTICS [*].—An original book on Optics by Prof. Schuster. This brief statement should be sufficient as a review, and no one interested in the science should require further inducement to at once add the book to his library. The book is not on geometrical optics, or systems of lenses, like those of Parkinson, Heath, and others; nor is it purely analytical like Airy's undulatory theory. The keynote of the book will be gathered from the following extract from the preface:—

> There is at present no theory of Optics in the sense that the elastic solid theory was accepted fifty years ago. We have abandoned that theory, and learned that the undulations of light are electro-magnetic waves differing only in linear dimensions from the disturbances which are generated by oscillating electric currents on moving magnets. But so long as the character of the displacements which constitute the waves remains undefined, we cannot pretend to have established a theory of light. This limitation of our knowledge, which in one sense is a retrogression from the philosophic standpoint of the founders of the undulatory theory, is not always sufficiently recognized and sometimes deliberately ignored.

So the book begins with an explanation of the Simple Pendulum and Periodic Motion, leading to the Kinematics of waves generally and wave-motion in the luminiferous ether, which is given as the definition of light. Geometrical Optics is represented by Fermat's principle of the path of shortest optical length and the principle of reversibility. The chapter on Interference is especially interesting; it describes Young's historical experiment illustrating the production of interference bands, and Fresnel's mirrors contrived for the same purpose. Newton's rings are explained with no very elaborate mathematics. We might thus go through the book, but it is unnecessary to give the headings of the chapters when we say that all the branches of the subject are exhaustively treated. Specially as relating to astronomy we might mention the chapter on "The Theory of Optical Instruments," which gives the *rationale* of images formed by a lens, the resolving power of a telescope with the formula, and of the magnitude of stars seen in telescopes of different apertures. A particularly valuable feature is the historical notes that Prof. Schuster gives at the end of some of the chapters. In the chapter, for instance, on "Theories of Light," there are short notices on the life and work of Cauchy, Green, Stokes, and Clerk Maxwell, in the last of which the following passage occurs:—

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[*] 'An Introduction to the Theory of Optics. By Arthur Schuster. London: Edwin Arnold, 1904. Price 15s.

> All mathematicians who, previous to Maxwell, had discussed the undulatory theory of Optics, started from the elastic solid theory of the luminiferous æther. That theory was able to give a satisfactory account of a great number of the phenomena of light, and was considered to be securely established. The phenomena of electricity were treated as independent facts, though no doubt many physicists held that ultimately electric action would be explained by the stresses and strains of the same medium which transmitted light. No one had, however, suggested properties of the medium different from those of an ordinary elastic solid. Maxwell attacked the question with great originality from another point of view. Having asked himself the question, what the properties of a medium must be in order that it should be capable of transmitting electric actions, he discovered that this electric medium was capable of transmitting transverse vibrations with the velocity of light. Maxwell then showed also how Fresnel's wave surface in double refracting media could be obtained by assuming that in such media there may be three dielectric constants, the polarization measured along their axes at right angles to each other being different.

As an educational work of the highest type in the latest developments of optics, the book may be recommended.

OBSERVATIONS D'ÉTOILES DOUBLES ET DISCUSSION DES MESURES*.—We are glad to welcome this extract from the *Annales Astronomiques* of the Belgian Royal Observatory. The measures and discussion are by M. G. van Biesbroek (Ingénieur des Ponts et Chausées), who had the use of the 15-inch refractor at Uccle. The programme was to observe pairs showing orbital motion, and more especially those in which the *comes* had apparently arrived at a critical position in the orbit, and also those difficult pairs requiring good atmospheric conditions. M. Biesbroeck seems quite satisfied with his surroundings. He says that the Observatory is built on a charming site to the south of Brussels, and on a hill which affords a very clear view of the horizon—free from noise, tremors, light, and dust. Altogether 358 pairs were examined, viz.:—

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* 'Observations d'Étoiles doubles et Discussion des Mesures.' Par G. van Biesbroeck, Ingénieur des Ponts et Chaussées. Bruxelles: Hayez, Rue de Louvain, 112. 1904.

209	W. Struve.	59	Burnham.
68	O. Struve.	22	Miscellaneous.

Judging from the W. Struve measures, which have received special attention from us, the work is of a high order, and we compliment M. Biesbroeck on being able, as a beginner, to measure, discuss, and publish so fine a series in eighteen months. The measures were made partly in 1903 and partly in 1904.

The Cyclones of the Far East.—In 1897 a work on "The Cyclones of the Philippines" was published in Spanish by the Rev. José Algué, S.J., Director of Manila Observatory, Philippine Weather Bureau. An English version, obtained through Dr. Bergholz's German translation, was very unsatisfactory, containing many scientific inaccuracies and some "sheer nonsense."

Father Algué has now amplified the scope of the work, justifying its more general title, and brought it out in English as a second (revised) edition.

In the first part of the book, on cyclones in general, we note that the author gives strong reasons for supporting the view that the fall of the barometer associated with a cyclone is the cause and not, as many have urged, the effect, but he does not devote much space to theorizing. The structure, anatomy, birthplace, and motion of rotation and translation are analyzed in a most careful and exhaustive manner, according to the time of year and the zone of origin and track, the number of principal types being no less than eleven. Much attention is also paid to accompanying barometric changes and to cloud-movements and cyclonic rain.

In the second part precursory signs are dealt with, cloud observations and photography, and variations of wind and air-pressure, including normals for different regions, the hurricane-wave (sometimes causing an elevation of 40 feet), and other indirect signs; many of those commonly associated with cyclonic disturbances are, however, shown to be quite unreliable (among them anticyclones, thunderstorms, and scintillation of stars).

The most important indications, however, are obtained by means of the author's invention, the barocyclonometer, which combines a special form of barometer with a cyclonometer or wind-disk. The special feature of the barometer is a movable rim, to be set according to a table engraved on it, in order to correct the indications for place and time of observation, the needle then indicating the distance of the centre of an approaching cyclone up to 500 miles. The cyclonometer is an ingenious contrivance with three needles, two pivoted at the centre of the disk and a small one near the end of one of the first, the use of which, carefully described with examples, is to determine by three successive approximations the direction of the centre of the cyclone.

The third part, on typical cyclones, gives detailed observations of several great cyclones of different types, giving a practical justification of the first and second parts, and also an account of some anomalies real (such as cyclones without motion, or without rain, or even without wind) or apparent (such as cases of simultaneous cyclones or bifurcated tracks).

The fourth and concluding part is of more direct interest to the sailor, giving hints as to his best course after determining the direction and distance of the cyclone centre, and also a list of harbours of refuge in the Philippine Archipelago and along the coast of China. It is admitted that for a vessel in motion the barocyclonometer is not so reliable as for one at anchor or for a fixed observatory, but the difference is not so great near the centre of the cyclone, so that its value appears to increase with its necessity.

It is a little disconcerting to find cyclones occasionally referred to as "meteors," an instance of the inconsistency of conventional terminology. The whole work is well illustrated with plates and charts, tables of local corrections, and other useful details, and, besides its immense practical value, is of wide general interest.

The Philippine Weather Bureau is to be congratulated on the energy of its head, which has enabled this work, involving an immense amount of labour, to be successfully carried out in spite of the "roar of cannon and the rumours of war." W. W. B.

AMONGST other publications received, we have 'Who's Who for 1905,' a work too well known to require description or commendation. It is not strictly astronomical, but we may say that it is found frequently useful for editorial reference. With it came the 'Who's Who Year-Book for 1905,' which is a directory of Government and other offices and officers, of societies and clubs, combined with a directory of the Press, whose use is obvious. Also from the same firm * we have received 'The Englishwoman's Year-Book and Directory,' which, in case its title is not sufficiently explicit, is mainly a directory of clubs and societies exclusively feminine, or in which ladies may be supposed to take an interest.

ALSO we have received from Mr. Hasse W. Tullberg, of Stockholm, 'Porträtgallerie der Astronomischen Gesellschaft.' The Astronomical Congress of the organization named was held last summer at Lund, and for some time previously Prof. Charlier, the Director of the Lund Observatory, and Dr. Engström, his assistant, had been gathering materials for this work, which results in an album (paper covered) containing portraits of 296 astronomers

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* Messrs. A. & C. Black, Soho Square.

from all parts of the world, with short biographies in German of the 350 members of the Gesellschaft. We can testify to the excellence of the portraits of the English astronomers and such of the others as we know, and it is interesting to look through the book and learn what sort of men are the others of whom we have only heard and read. The work will be forwarded immediately post-free on receipt of 10*s.* 6*d.* by the publisher, Mr. Tullberg.

NOTES.

Comet Notes.—A new comet (1904 *d*) was discovered by M. Giacobini at Nice on Dec. 17, making, we believe, his seventh cometary discovery; it was of the 11th magnitude, and was in R.A. $16^h\ 14^m\ 40^s$, N. Dec. 27° 28′. Herr Ebell has deduced the following elements and ephemeris (for Berlin midnight):—

T........	1905 Jan. 3·28 Berlin M.T.
ω........	75° 10′
☊	225 1
i........	103 27
$\log q$	0·2717

1905.	R.A. h m s	N. Dec. ° ′	Brightness.
Jan. 3	17 2 37	36 8	1·22
7	17 16 35	38 28	1·26
11	17 31 55	40 55	1·30

It will be seen that the motion is retrograde, and the inclination large, so that it is unlikely to be a periodic comet. The perihelion distance is large, and the distance from the Earth, though diminishing throughout January, is considerable. Consequently the comet will probably never become brighter than the 9th or 10th magnitude.

Nice Observatory has also distinguished itself by picking up Tempel's Second Periodic Comet, in spite of its faintness and unfavourable position. It was found on Nov. 30 by M. Javelle, being $3^s{\cdot}9$ west and 4″ south of the place predicted by M. Coniel from M. Schulhof's elements; this is a wonderfully close prediction for a comet, and reflects great credit on M. Schulhof. His elements, which were given in the *Observatory* last September, must be almost correct. The period is 5·2786 years, and perihelion passage took place 1904 Nov. 10·45.

Ephemeris for Paris Midnight.

1905.	R.A. h m s	S. Dec. ° ′
Jan. 1....	21 33 24	20 50
5....	21 46 50	20 1
9....	21 59 59	19 9
Jan. 13....	22 12 49	18 14
17....	22 25 29	17 17
21 ...	22 37 47	16 19

Encke's Comet was visible in an opera-glass early in December; it is now lost in the sunlight, and on its emersion will be best seen in the southern hemisphere. A. C. D. C.

OBITUARY.—ADMIRAL SIR ERASMUS OMMANNEY. The record of the death of this gallant officer, which took place on December 21, may fitly find a place here, for he was well known among astronomers and in their science he took a great interest. For many years he attended the Greenwich Visitation regularly, and so late as two years ago, despite his great age, made a point of ascending to the highest floors to see the additions, if any, to the equatorials, and otherwise showed his interest in the work of the place. It is not necessary here to detail the services he rendered to his country in the exercise of his profession, but it may be mentioned that he saw service at the age of 13 years in the war with Turkey which resulted in Grecian independence. He was also an Arctic traveller and went with Capt. (afterwards Admiral Sir) James Ross to relieve the whaling-vessels icebound in Baffin's Bay, and again in 1850 Capt. Ommanney was second in command of the force which set out to find Sir John Franklin.

Erasmus Ommanney was born in London on May 22, 1814, being the seventh son of Sir Francis Molyneux Ommanney, some time M.P. for Barnstaple. He entered the navy in 1826, and retired under the age clause in 1875. In 1867 he was created C.B., became a knight in 1877, and was promoted to K.C.B. in 1902. He joined the Royal Astronomical Society in January 1851, and had contributed to the 'Monthly Notices' papers on the Aurora Borealis, on the Perseid Meteors, and observations of the Transit of Venus 1874, which he observed from Luxor, Egypt, in company with Dr. Auwers and Dr. Dollen. Sir Erasmus Ommanney was a Fellow of the Royal Society, having been elected in 1868.

REV. J. M. BACON. This gentleman, who had been a Fellow of the Royal Astronomical Society since February 1888, died, we regret to say, on Christmas night last at Coldash near Newbury, at a comparatively early age. He was born in 1846, and was educated at Trinity College, Cambridge, of which foundation he was a Scholar. Having been ordained priest in 1871, for seven years he held a curacy at Harston, Cambridgeshire, but had not taken regular clerical duty for many years past. Instead of this he devoted himself to scientific experiments of a rather original kind. A paper that he read before the Astronomical Society in June 1898, on some experiments on the "Actinic Qualities of Light as affected by different Conditions," will be remembered, the gist of the paper being that better photographs can be taken in moist than in dry air. For the purpose of acoustic experiments he made balloon ascents, and published accounts of his journeys in his books 'By Land and Sky' and 'Dominion of the Air,' and gave lectures on the same subject. Mr. Bacon was a well-known member of the British Astronomical Association, and went on three expeditions organized by that body to observe eclipses—to Norway in 1896, to India in 1898, and to North Carolina in 1900. His ascent in a balloon in company with his daughter, Miss Gertrude Bacon, to observe the Leonid meteors, which ended almost in a tragedy, will be remembered.

THE LIVERPOOL ASTRONOMICAL SOCIETY.—The Annual Report of this amateur society for the Session 1903–4, lately received, contains an interesting address by the President, Mr. W. E. Plummer, on "Planet Markings," and another, also by him, urging amateurs to observe variable stars, which, he assured them, would be an attractive kind of work, and would be valuable in forwarding an important branch of astronomy. The Report also contains short papers by Rev. S. J. Johnson and Father Cortie, as well as short reports of the Monthly Meetings. The members of this Society have the privilege of attending at the Observatory every Thursday evening to receive instruction in the use of the equatorial. It is proposed to form a party under the auspices of the Society to observe the Solar Eclipse on the north coast of Spain in August 1905.

THE PARIS—GREENWICH LONGITUDE.—Our readers will remember that in the year 1902 we published notes at intervals recording the progress of the work of the determination of the difference of longitude between Paris and Greenwich, which was then being made by two French observers, M. Bigourdan and M. Lancelin, who replaced M. Renan when the latter fell ill, in co-operation with two of the Greenwich staff who were making an independent determination of the same arc.

In the *Comptes Rendus* of the Paris Academy for December 12 M. Lœwy gives the result found by the French observers. By referring to the notes above mentioned, it will be found that the whole operations consisted of two parts—one in the spring, one in the autumn,—each consisting of three series, so that the observers exchanged stations five times. In all there were thirty-five nights on which the longitude was determined by MM. Bigourdan and Lancelin. The observations on two of the nights, Sept. 22 and 24, give an exceptionally large value, differing about $0^{s}\cdot2$ from the mean, and another result, that on May 4, is similarly small, but, excepting these, which scarcely influence the final figures, the accordance is very satisfactory and the mean result arrived at for the difference of longitude between the meridian of Cassini and that of the transit-circle at Greenwich is

$$9^{m}\ 20^{s}\cdot974, \text{ with a probable error of } \pm\ 0^{s}\cdot008.$$

The exact result found by the English observers is not yet formally announced, so that we are precluded from quoting this, but we happen to know that the figures given by the Astronomer Royal in his Report for 1903 are not sensibly altered, so that the result is about $9^{m}\ 20^{s}\cdot94$, which is in satisfactory agreement with that given by M. Lœwy, who gives his personal conviction that the values found in 1902 fix definitely the difference of longitude between the fundamental meridians of Greenwich and Paris. It will be noted that these are about midway between the values found by the French and English observers in 1888 and 1892.

ZONE-TIME SYSTEM.—On and after October 30 last the Time-ball at Hongkong has been dropped by order of the Governor of the Colony at $17^{h}\ 0^{m}\ 0^{s}$ G.M.T. which is $23^{m}\ 18^{s}\cdot14$ in advance of $1^{h}\ 0^{m}\ 0^{s}$ Hongkong Mean Time. The circumstances which have led to this action are referred to in our number for November last.

THE TOTAL SOLAR ECLIPSE OF AUGUST 30 NEXT,—Expeditions are beginning to be organized to observe this phenomenon. At a meeting of the Société Astronomique de France, the Comte de la Baume Pluvinel gave an address on the subject and mentioned the intentions of American astronomers to send observing-parties to Labrador, Spain, and Upper Egypt. The British Astronomical Association and the Liverpool Astronomical Society are arranging expeditions, as will be seen on pages 49 and 69 of this number. Messrs. T. Cook & Son, the tourist agents, have sent us the programme of a Select Conducted Tour to Burgos, arranged to leave London on Friday, August 25, for the Eclipse of the Sun. The fare to cover all necessary expenses from London and back, with accommodation at the best hotel in Burgos, is thirty guineas.

A POSSIBLE VARIABLE STAR.—In a letter to *Nature* of November 17, Mr. Gore describes an observation of Atlas (27 Tauri) made by him on October 29 last about 9 P.M. G.M.T., when he saw this star slightly fainter than Pleione (28 Tauri). Now Atles is given in most catalogues as being considerably brighter than Pleione, so Mr. Gore looked again at the first opportunity, which was on November 9, and then found 27 Tauri more than a magnitude brighter than its southern neighbour. He suggests that Atlas may be an Algol variable.

LONGITUDES BY WIRELESS TELEGRAPHY.—Prof. Albrecht, of the Prussian Geodetic Institute, has been making some experiments with a view to finding whether it is practicable to determine telegraphic longitudes without wires. He set up installations at a distance of 33 kilometres apart, and with Herr Wanach exchanged signals between the stations, using different kinds of coherers and different quantities of energy. The experiments appear to have been successful, and the average time of transmission for the double journey to be about $0^{s}\cdot008$.

THE EROS PHOTOGRAPHS.—In a note in *Nature* of Dec. 15, Prof. Turner points out a rather serious discrepancy he has noted in the Eleventh Eros Circular which has recently appeared. This work, although called a "circular," is actually a quarto volume of 400 pages and contains the places of reference and comparison stars derived from the photographs taken at various observatories. Comparing the places of the same stars as found from plates taken at Paris and Algiers, there are found the following differences in the mean of 5 groups of 87 stars each:—

Mean Magnitude.	Difference.	Mean Magnitude.	Difference.
	″		″
8·8	−0·27	11·2	−0·72
9·4	−0·42	11·6	−0·83
10·4	0·57		

It has been hoped that photographs would be free from "magnitude equation," but it seems from this that it exists in a most pronounced form. It is suggested that this may be due to an error of centering of the crown lens relatively to the flint. The stray light on one side of the image which would be the result of such an error would not be strong enough to affect the sensitive film in the case of faint stars, but for a bright star it would spread the image in that direction, and so introduce a spurious displacement of the centre.

THE WEATHER IN DECEMBER.—The rainfall at Greenwich in December amounted to 2·25 inches, which is about half an inch above the average for the month taken for the fifty years 1841-1890. This brings the total for the year to 20·66 inches, the yearly average for Greenwich for the same years being 24·54. Despite the excess of 17 inches in 1903, the deficit of rain at Greenwich during the twelve years ending 1904 December 31 is about 21 inches. The characteristic feature of the month was the fog which prevailed during the last part of the month. The barometer-reading was above 30 inches almost continuously from the 17th to the 29th.

A CORRESPONDENT of the *Standard*, Mr. S. C. Russell, has been observing α Orionis (Betelgeuse) during the past three months to detect changes of magnitude. Using Aldebaran and Capella as his comparison stars, on Sept. 21 it was estimated as bright as the first of these. In October and November, α Orionis varied very little in brightness, rising just a little above Aldebaran, and at the beginning of December it was noted as just below Capella.

THE veteran astronomer M. Janssen appears to have had a narrow escape, judging from a note in the daily press. It seems that he went to the summit of Vesuvius lately for the purpose of studying volcanic vapours and gases, and was saluted by an eruption, in the course of which he was nearly struck by an immense stone. Despite the smoke and cinders, he took some photographs, possibly spectrographs, and sent them to Meudon.

Just as we are going to press, information comes to hand of a new comet, the third of the year, excluding the re-observation of Comets Encke and Tempel, discovered by M. Borrelly at Marseilles on Dec. 28. Its place for Dec. $29^{d}\cdot365$ G.M.T. is given: R.A. $1^{h}\ 13^{m}\ 40^{s}$, increasing 96^{s} daily; N.P.D. 100° 0′, decreasing 54′ daily.

Sir Norman Lockyer has been elected a Corresponding Member of the Imperial Academy at St. Petersburg.

The next Meeting of the Royal Astronomical Society will be on Friday, January 13; of the British Astronomical Association on Wednesday, January 25.

From an Oxford Note-Book.

[*Interpolated* 1904 *December* 23.—May I again interrupt the regular flow of the notes of travel to mention a piece of news which came this morning by telegram and which will, I am sure, be of general interest? It is to the effect that the Trustees of the Carnegie Institution have seen their way to a very considerable subsidy for the new observatory on Mount Wilson. I will not mention the figures, as there may possibly be some mistake; but I am pretty sure that all the experiments which Prof. Hale has in mind will be carried out, including the erection of Mr. Ritchey's five-foot mirror in the splendid climate on the summit of Mount Wilson. All seasonable greetings to the new Monastery!]

On the way Eastward, Sept. 14–18.

A faultless blue sky, a "leopard" mountain spotted with dark green trees on rich orange-brown slopes, and the white buildings of the Observatory on the summit! These are the elements, grouped in every varying fashion, of the view which holds the traveller's gaze as he winds up the latter half of the 27 miles' journey from San José to the Lick Observatory. Is there any astronomer living who can make that journey for the first time unmoved? There has been about the Lick Observatory from its foundation a halo of romance, which has not faded with years, nor with the more recent erection of other observatories. Indeed the foundation of the latter has added a classical touch to the romance. Coming fresh from the newest experiment upon Mount Wilson, or even looking back to our visit to the Yerkes Observatory, the older-established institution has an air of maturity if not almost of venerability. The blue sky reminds us that the climate which it is proposed to make use of on Mount Wilson has been in possession of the Lick Observatory for 16 years. When the great dome is reached and we see the 36-inch telescope, the striking similarity to the 40-inch reminds us that we are in presence of the original—an original so satisfactory that nothing better could be done than to copy it. Even the wooden stands on the floor, which so conveniently receive the spectroscopes when not in use on the telescope, are the prototypes of those which have been adopted elsewhere. Sixteen years is not a long life for an observatory (even the Oxford University Observatory can proudly claim an antiquity nearly twice as great), but they have made history very fast on Mount Hamilton!

THE journey thither is a leisurely performance, especially after the second change of horses at Smith Creek. There are no less than 365 turns in the remainder of the road up to the summit, as the driver tells you. Some of them are dignified by special names, such as Hairpin Turn, the Devil's Elbow, "Oh! My" Turn, the last being a place where a sudden precipitous view causes ladies to exclaim in the manner indicated. If the driver thinks you a suitable person, he will also casually remark that in Leap Year there are 366 turns, and when (being a suitable person) you enquire how that comes about, he replies that in that year he always drives over the edge, so that the stage turns over and makes the odd turn. He has many lonesome journeys on which to think out these little pleasantries, and it made our pulses beat quicker to find that he had known Hank Monk, and could tell how he drove Horace Greeley. With the way so beguiled and continual new views of the mountain to delight the eye, the six hours' drive did not seem long. But coming down a few days later was another matter. We went whizzing round the turns, keeping nobly on the very outside edge; clods of oil-caked road saluted us gaily at each whirl of the wheels; and we were back in just half the time required for the ascent.

THE Observatory is on the very top of the mountain, so that you can find your way there, even in a fog, by continually ascending. [This lesson is taught to those who live on the mountain in early infancy; it is the parents here who instruct their children to "go up," and not, as in the days of Elisha, the children who give their elders this advice.] The summit being convex, a portion had to be shaved off to get enough room for the Observatory buildings; but unfortunately they did not shave enough to leave room for the golf-links. Owing to this oversight the links and the lawn-tennis court are somewhat lower down, and the former has suffered a little in convenience; but with two clubs (one to drive up-hill and one to drive down) it is possible to get much exercise and some amusement from the golf. It is advisable, however, to play with a native, or you will probably lose your ball every shot;

though, if you are fortunate enough to be accompanied by one of the observers, especially one of those familiar with motions-in-the-line-of-sight, he will infallibly find it for you. One of the most interesting hazards is a stack of bricks, which reminds us that when you are building an observatory at an altitude of 4000 feet, it is cheaper to make the bricks on the top than to carry them up; so that a brick-kiln takes its place among the adjuncts of a modern observatory.

THE chief difference between the 36-inch and the 40-inch is that the former stops work at dawn, not having any solar work to do. But then it does not get so much rest from cloudy nights, of course, which tends to restore the balance. The devotion of the community to the big telescope is the same in both cases, and if we could only search the annals of the remote past we might find that this excellent spirit was carried as a tradition from one place to the other. Passing to matters of detail, it is noteworthy that the movements of the younger dome and floor are more sprightly, and no watchful engineer-in-charge has yet been deemed necessary for controlling the sober pace of the elder structures. But in both cases the ease with which the huge machine is worked is extraordinary, and immense credit is due to Messrs. Warner and Swasey for these two marvels of engineering. It must be a great satisfaction to them to note the ever-increasing pile of work done by these two great telescopes, and to know that it has been accompanied throughout by the blessings, and not the curses, of the observers who toil the night long, and to whom a flaw of construction would mean a misery.

PROF. HUSSEY was kind enough to devote part of one of his nights to a search for Phœbe, the ninth satellite of Saturn. Two or three faint objects were seen near the place, but it was practically impossible to measure their positions and so to detect any motion. If one of them was the satellite, it would almost seem that visual observations of it, except of a very rough kind, will be impossible. We had also an opportunity of seeing some of the close doubles under observation—some of them too close for me to see. Messrs. Hussey and Aitken are making a complete survey of the northern sky (down to −20° Dec.) to the 9th magnitude for new doubles. They verify the known doubles as they go along, amounting to about 3 per cent. of the whole number examined; and they find rather more than 3 per cent. of new doubles in addition. The whole survey represents a magnificent piece of work, and it is to be hoped that it may be completed by extension to the remaining quarter of the sky in the Southern Hemisphere.

THE Crossley Reflector has just been provided with a new and very strong mounting. It is delightful to hear the enthusiasm with which Prof. Perrine speaks of this instrument and to realize what he will probably do with it. We have just had news that Common's five-foot is to be set up at Harvard, on the shores of the Atlantic, with every prospect of an honourable career. And here, close to the Pacific, is another instrument which was once Common's. It is true that not a vestige of his original instrument now remains—mirror, dome, mounting have all been successively replaced—and it is true also that the gift was made by another; but I hope it hurts no one to recall that the initiative of making a 3-foot reflector was Common's, and that it is ultimately due to him, in one limited sense, that this fine instrument is on Mount Hamilton. Now that we have lost his cheery presence it is some consolation to reflect how widespread an influence on the work of the future may be traced to him.

Prof. Perrine kindly showed me the original negative of the spectrum of the nebulosity round Nova Persei—a negative I have regarded from a distance with awe ever since I heard of its being taken. And I also saw the spectroscope specially designed for this work, which has produced other marvellous results in the way of photographing very faint objects. Prof. Perrine thinks an even better one could be made, and I hope it will not be long before he gets the opportunity to try his hand. The polariscope negative of the Nova Persei nebula did not altogether convince me that there is no polarization. I hope I may be allowed to state this opinion frankly. The two pictures are so faint that, although they appear to the eye nearly equal, I should not like to be sure that there was not a difference of (say) 30 per cent., and this might be enough to fit in with the theory of reflected light, if the incidence were not too great. This is, of course, only a personal opinion.

On one morning we were awakened either by the rising sun shining into our windows or by the winding of the hunter's horn. We had to imagine the noble cavalcade of gaily-dressed hunters and huntresses, for we could not see a sign of them. Mysteriously they slipped out into the early morning to chase the deer, and as mysteriously returned, no man knowing whither they went or where the quarry was slain. Indeed, some of the unrighteous hinted that they consisted simply of one man and a gun, and that we must have dreamed even the sound of the horn. However that may be, certainly venison appeared at our table and at other tables on the mountain: and certainly it was good to eat: and certainly a head with a pair of antlers left the observatory by the next stage to be properly mounted in San José. I wonder how many living astronomers there are who can not only shoot a buck, but carry half of him 5 miles up a mountain?

We had not much time for San Francisco, with its Golden Gate, and its China town, and its tramcars which go upstairs. And even less for the Berkeley University, though we had opportunity to realize what we were missing. There is at Berkeley a wonderful open-air Greek Theatre, where a whisper on the stage can be heard by thousands of people. Here came President Roosevelt the other day and addressed some 10,000 to general satisfaction. The students at Berkeley affect a curious head-gear. A top-hat is first painted with elaborate designs and then sat upon : the result is then considered the proper covering for less formal occasions, *e. g.* when working in the laboratories or workshops. We learnt, too, that no less than 17 of the staff of this University were about to start for the Congress at St. Louis—a journey similar to that from Oxford to Moscow, say. But perhaps the most interesting experience at Berkeley was that of talking to the genial President of the University for quite half an hour and hearing no expression of anxiety as regards finance. It was some little time before I realized what familiar element was missing from the conversation : but when I did so, and ventured to put the obvious question, I learnt that it was a State University, which had a definite subsidy of 10 cents in every so many thousand dollars of State revenue ; and the prosperity of the University was hence as definitely assured as that of the State itself, which in California means a good deal. I further learnt that the State control, which is a necessary concomitant of State aid, is, at any rate in this instance, no inconvenience. If only Oxford and Cambridge could make up their minds not to be afraid of bogeys, and to cheerfully reply to the question put by the Chancellor of the Exchequer this summer, "How far, if you have State Aid, will you accept State Control?" that they would accept what was reasonable, might we not soon put an end to the present poverty of these Universities and others in England?

There is a fine "Students' Observatory" at Berkeley, with Prof. Leuschner at the head. Besides more obvious work of various kinds, he has a small staff of computers at work on the orbits of the minor planets, especially those discovered and endowed by Watson. He was in the middle of an exciting chase after a "missing" planet, and full of hopes that he had found the encounter which had changed its orbit. He had more exciting suspicions still, but it would be unfair to mention them until he has had a better chance of seeing whether they are well founded.

We have seen the Mormons, and bathed in Salt Lake, where your feet come out of the water when you try to swim : and the train has ascended 10,000 feet and come down again, without any particular mishap : and we are getting within measurable distance of St. Louis. It seemed as though we could tell that by the temperature, for it was 96° in the car ; but then came a welcome storm of some sort, and the thermometer fell 20° in as many minutes : so we *may* not be broiled after all the prophesies to that effect.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1905 January 13.

Prof. H. H. Turner, D.Sc., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
E. T. Whittaker, M.A.

The Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker announced that sixty-six presents had been received since the last Meeting, among them being a further series of enlargements of Astrographic Chart Plates from the Royal Observatory, Greenwich, and a copy of Sir Robert Ball's new book, 'A Guide to the Heavens.'

The President. We have not many papers suitable for reading this evening, but there are some among them which I hope will give cause for some discussion. Mr. Cowell has another paper on his lunar work, and we have also a paper on a similar subject by Prof. Newcomb, which I will also ask Mr. Cowell to take charge of, and after these we will take the papers which relate to solar matters.

Mr. Cowell described *Prof. Newcomb's* paper on "The Eclipse of Agathocles." Prof. Newcomb had returned to the eclipse of Agathocles, B.C. 307, and was now in practical accordance with Mr. Nevill's view (M. N. xxxix. p. 73) that the ancient eclipses on the whole supported the theoretical value of the secular cceleration of the Moon's longitude. Continuing with a de- ription of his own paper, Mr. Cowell said that this concluded scussion of the Moon's longitude. He obtained a value for of the Moon's perigee 15″ per century greater than h Prof. Brown had quoted as the observed value. oretical values were in closer accordance with the observed value, according to Hansen, than with the one deduced in the present paper.

Mr. Crommelin. Mr. Cowell showed me Prof. Newcomb's paper before the Meeting. I think it should be mentioned that the first point of the paper is that a second point on the track of totality has been found in an ancient writer, who states that the eclipse was total at the Hellespont and gives its magnitude at Alexandria. We have thus an unusual amount of material for such an ancient eclipse. The position of Agathocles is so uncertain that his observation contributes nothing to our knowledge of the secular acceleration, but it appears that the totality track can only be brought up to the Hellespont by the use of a small value of the acceleration, so that if the record is reliable it seems decisive in favour of a value near the theoretical value.

Mr. Cowell. I ought, of course, to have mentioned this. The omission was due to my being more interested in the conclusion than in the methods.

Mr. Cowell then read extracts from Prof. Newcomb's paper, and said in reply to Mr. Lynn that Cleomede was the writer referred to.

Father Cortie. The first and most important conclusion of Mr. Maunder's recent paper is that "the origin of our magnetic storms lies in the Sun, not in any body or bodies affecting both." This conclusion he proves by showing that the magnetic disturbances on Earth mark out the synodical rotation-period of the Sun in the sun-spot zones, and, moreover, he supposes that the magnetically-active regions on the Sun, which are associated with sun-spots, act on the Earth by means of streams of particles ejected from those centres which impinge on the Earth at each successive synodical solar rotation. My criticism of this position is two-fold. First, I contend that the conclusion advanced by Mr. Maunder does not logically follow from his premisses; and, secondly, with regard to his premisses, I will endeavour to show that if he associates his sequences of magnetic storms with sun-spots, and supposes that the mode of operation of the magnetic centres is by means of stream-lines of ejected particles, that such an assumption leads to a dilemma, so that either magnetic storms are dissociated from sun-spots, or, if they are associated with them, that the mode of causation is not by stream-lines ejected from sun-spots as centres of magnetic action. And first as to the logic of Mr. Maunder's conclusion and argument. I presume that we are seeking for the efficient cause of magnetic storms on Earth—that this is what Mr. Maunder means by the term "origin of our magnetic disturbances,"—and not merely for a condition. Mr. Maunder's first premiss in his argument is that magnetic storms mark out the synodical rotation-period of the Sun, and his second premiss is that phenomena which mark out the synodical rotation-period of the Sun have their origin or cause in the Sun, and therefore that magnetic storms have their origin or cause in the Sun. But even if we allow that Mr. Maunder has proved his first premiss up to the hilt, the second premiss is not necessarily true, and therefore the conclusion of the argument is faulty. If two sets of connected phenomena are perfectly concurrent, it does not logically follow that either one is the cause of the other, that A causes B or B causes A, for there is a third alternative—that both A and B may have their cause or origin in C. This is Father Sidgreaves' position, that both magnetic storms on Earth and sun-spots on the Sun have their cause in something, viz. swarms of electrified particles, which are neither on the Sun nor on the Earth. So that Mr. Maunder's argument leaves this theory just where it was.

I now proceed to Mr. Maunder's table of sequences, on which is founded his first premiss, that magnetic storms mark out the synodical rotation-period of the Sun. I studied some of these sequences set down in Tables III., VI., VIII. of his paper in connection with the sun-spots with which they were presumably associated. There was not time to study them all, so, beginning with the first sequence in Table III., this was studied, as also number 2, and incidentally numbers 4 and 23, as also the important sequence 32. Charts of the sun-spots and of the magnetic storms were drawn up, similar to that published in Table A of Father Sidgreaves' paper (Mem. R. A. S. vol. liv.), and utilizing materials prepared for that paper. [Photographs of two of these charts, one of the sun-spot group 1892 July 4–November 1, visible during five rotations, and the other of the sun-spot group 1886 March 29–September 15, visible during seven rotations, were thrown on the screen, and the construction of the charts was explained.] These charts show great anomalies if we are to associate sequences of magnetic storms with sun-spots. For instance, the greatest magnetic storm of 1886 occurred when the sun-spot of March 29–September 15 was first seen on the Sun's E. limb, and yet at the successive rotations of the spot to this same position relatively to the Earth, for five rotations, the presumed magnetic centre associated with the sun-spot remains inactive. But at the seventh rotation, when the spot-group is very small and solar activity in general has much diminished, the series of four storms, forming sequence 23 of Mr. Maunder's list, begins to appear. Why an active magnetic centre, associated with the most energetic phase of a sun-spot's life-history, should remain dormant for five rotations, to appear again as active when the sun-spot was vanishing, is at least puzzling, if the magnetic storms are associated with sun-spots. Mr. Maunder's sequence no. 2, consisting of three successive storms which marked the synodical rotation-period of the Sun, occurred when the spot under discussion was on the W. limb of the Sun, at its fourth, fifth, and sixth returns. Now, if the mode of action by which sun-spots cause magnetic storms on Earth is by the projection of stream-like lines of particles which reach the Earth at its successive returns to the same position relatively to the Sun, it is evident that these streams cannot impinge on the Earth when the spot occupies an E. limb position on the Sun, and likewise a W. limb position also. So that a series of instances like this one shows that either magnetic storms are dissociated from sun-spots, or that the mode of action by which they produce magnetic storms is not that adduced by Mr. Maunder. The longest sequence in Mr. Maunder's list is no. 32, of July 17–November 29, 1889, containing six members. By means of this sequence he endeavours to connect the two magnetic storms of 1889 November 1 and November 26–29, which, as Father Sidgreaves pointed out, occurred at a time when the Sun was entirely free from spots, with the magnetic storm of July 17, which occurred during the second appearance of the greatest spot of this year of minimum solar activity. But from a detailed study of this sequence in relation to the sun-spots visible during the months it covers, it would appear that its members are not connected with the June spot, but the November storms are more likely, if associated at all with sun-spots, to be connected with a sun-spot group of 1889 August 2–October 23. Another point with regard to this sequence. Mr. Maunder brings it forward as showing a more rapid rotation-period of the Sun's equatorial regions than the mean rotation-period of 25·38 days, precisely because it was associated with spots occurring in low solar latitudes at the minimum. But it is more likely that three at least of the storms of this sequence were associated with a spot in latitude −22°, which is relatively high to lat. −7°, and lat. −8°, the two other groups of spots with which the three other storms are associated having appeared in these solar latitudes. Internal evidence, too, of the various members of this sequence being connected with not one, and that the June spot, but with several spots, appears from the change of nearly 52° in the longitude of the centre of the Sun's disc during the progress of this sequence. In conclusion, although admitting the reality of the sequences in several cases, it is my present opinion that they are not particularly associated with sun-spots, and that the mode of action of magnetic centres on the Sun is not by means of stream-lines of ejected particles. Rather I would incline to the view that general activity on the Sun is associated with magnetic storms, not as a cause but as a condition.

The *Astronomer Royal* then took the Chair.

Prof. Turner. Prof. Schuster, I am sorry to say, is not able to be present himself, so he has asked me to take charge of his paper, and I shall do the best I can to explain the points. Prof. Schuster's paper may, I think, be divided into two parts. In the first place there is the definite statistical question, whether Mr. Maunder's contention that the rotation-period of the Sun is represented in the magnetic storms is borne out by facts. Prof. Schuster has examined the statistical question by his particular method of the "periodogram" and comes to the conclusion that Mr. Maunder's contention is confirmed, but with some remarkable modifications which I will presently mention. The other part of Prof. Schuster's paper deals with the theory which Mr. Maunder offers in explanation of this fact. He does not find himself at all in agreement with Mr. Maunder in his physical explanation of the statistical fact, and he gives reasons for adopting a totally different view of the origin of magnetic storms. He resumes the discussion as to the energy which is available for the production of magnetic storms and gives reasons why he cannot admit that Mr. Maunder has removed the difficulties to which Lord Kelvin called attention. Prof. Schuster's own view is that the energy for the production of these storms must be drawn from the Earth's rotation. After examining one or two other suppositions with which I will not trouble you, he comes to the conclusion that the one remaining source of energy, the diurnal rotation of the Earth, is sufficient for our purpose. "The energy drawn away from the Earth would gradually diminish its rotational velocity, but so slowly that after provision has been made for the supplying of magnetic storms during a million years, the Earth, as a time-keeper, would then be losing at the rate of only one second per year." So that we should not be able to detect the stopping of the Earth's rotation if it were supplying energy for these magnetic storms. "Having been led to what seems to be the only available source of energy we must try to form some idea as to the manner in which the frequency of the magnetic storms may be influenced from outside, while their primary cause lies within the terrestrial atmosphere. It is important for this purpose to keep in mind that the intensity of an electric current always depends on two things--electromotive force and resistance." A sudden change in either of these elements will produce a storm, and Prof. Schuster inclines to the second (the resistance) as involving fewer difficulties than the former. "When he discovered, in 1887, that a gas may artificially be brought into such a state that it ceases to behave as an insulator even to the smallest electromotive force, he at once pointed out the bearing of this on some phenomena of terrestrial magnetism, and suggested, in his first publication on the subject, that the greater amplitude of the diurnal variation may be accounted for by the greater conductivity of the outer regions of the Earth's atmosphere when the number of sun-spots is great*. Returning to the same subject in the concluding portion of his Presidential Address to Section A of the British Association at Edinburgh, he further suggested that both the periodicity of sun-spots and the connection between these spots and magnetic disturbances on the Earth may be due to a periodically recurring increase in the electric conductivity of the parts of space surrounding the Sun. Since then the possibility of corpuscular projections has been discovered. Such corpuscular projections do, in passing through gases, invariably increase the electric conductivities. Ultra-violet light acts in the same way, provided it falls on sufficiently large conglomerations of matter. Without forming, therefore, any very definite theory we may accept the view that there is some solar effect propagated in straight lines which may increase the electric conductivity of the atmosphere, and therefore set a magnetic storm going without supplying its energy." For the manner in which this may be effected I must refer those interested to the paper itself, which is already in type and of which there are some copies in the room. Prof. Schuster's attitude with respect to Mr. Maunder's paper is summed up in the concluding sentence, which is, perhaps, a stronger testimony to the merits of the paper in that Prof. Schuster does not altogether agree with one half of it. He says:—"I cannot, therefore, agree with his somewhat boastful claim that he has rendered clear what Lord Kelvin has called a 'fifty years' outstanding difficulty.' He has, no doubt, added a new fact and made an important contribution to the subject. He has given renewed interest to it and brought out the urgent importance of further investigation, but the mystery is left more mysterious than ever; the facts have become harder to understand and more difficult to explain."

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* Proc. Roy. Soc. xlii. p. 371 (1887).

I now return to the statistical part of the paper, and as the method of the periodogram is not very well known, those already familiar with it will no doubt forgive me for explaining it in some detail for the benefit of others. Prof. Schuster's chief paper on the subject is entitled "The Periodogram of Magnetic Declination" and is published in the Trans. Camb. Phil. Soc. vol. xviii., the "Stokes Volume." There is another account of the method in 'Terrestrial Magnetism' for March 1898 ("On the investigation of hidden periodicities with application to a supposed 26-day period of the Meteorological Phenomena"), but neither of these papers is very accessible, and I may therefore begin from the beginning. Suppose we have a series of observed quantities, say monthly rainfalls; and we wish to test whether there is an annual period, *i. e.*, a periodicity which recurs after every 12. We divide 360° into 12 parts, and multiply the series in order by sin 0°, sin 30°, sin 60°, sin 90°, &c., and add together the results. After 12 terms the factors recur, so that it is simpler to add together the original numbers after every 12 terms before multiplying. We divide the total by the sum of the squares of the factors and we get a result which we may call a. If there be no annual period, a should ideally be zero; but in practice we shall generally get a sensible value for a though it may be small. Next we multiply the original series by the cosines of the same angles, cos 0°, cos 30°, cos 60°, &c.; divide by the sum of squares, and we get a result b, which again will be small if there is no real annual term, but will not in practice be zero. Finally, we form the quantity $I=a^2+b^2$. This is the familiar Fourier analysis. Now to form a periodogram we go through this operation a number of times, taking next not 12 terms but 13, and therefore using the angles formed by dividing 360° into 13 parts. Then we divide into 14 parts, and so on. Again, we should also go backwards from 12 and take 11, 10, and so on. It will be seen that a good deal of numerical labour is before us, but we shall see the value of it presently. Moreover, we must devise some method for filling in the gaps between the subdivisions of 11, 12, 13, &c. &c., so as to get values of I corresponding to periods which do not contain an integral number of months exactly. This point I cannot touch upon here: but I will ask you to consider that we have found a^2+b^2 or I, for all possible periods, or at least a very large number of them; and now we form the periodogram by plotting this quantity I as ordinate against the period as abscissa. The ordinates will in general be small, but when there is a real periodicity they will shoot up to a pinnacle. Now there is a beautiful analogy between this diagram and that representing the intensity of a spectrum, as plotted against wave-frequency. The spectrum should be that of a gas, giving bright lines, mixed with impurities which show a faint continuous spectrum; and the intensity of this continuous spectrum corresponds to the accidental values of I obtained when there is no real periodicity; while the large ordinates or pinnacles show where there are true periodicities or bright lines. This analogy is no mere accidental similarity, but a complete mathematical one; the mathematical expressions representing the facts in the two cases are precisely the same, and we are thus able to apply known optical theorems to the periodogram. Let us consider a few of the parallel features. A double line in the spectrum corresponds to the existence of two periodicities in the periodogram. The two lines may be close together, but arising from different substances, as in the case of D_2 and D_3. Similarly we may have two ordinates in the periodogram close together, one corresponding to the solar-synodic rotation, and one to the Moon's sidereal revolution. If we wish to separate them in the spectrum we must take care that we have sufficient "resolving-power"; and the analogy for resolving-power in the periodogram is the *total length of the series of observations available.* This is one of the most valuable results of the correspondence, from the way in which it gives us a simple and sound view of what can be done with our material. If we have not a long enough series, we cannot separate the lunar effect from the solar effect. I may say here that Prof. Schuster does find that the series of magnetic storms tabulated by Mr. Maunder is long enough; he can separate out the lunar effect, and finds that it is negligible, which is a noteworthy result. Returning to the analogy, a band in the spectrum means a number of lines side by side and corresponds to a number of ordinates side by side in the periodogram such as would represent, for instance, the different (but nearly equal) rotation-periods of sun-spots in different latitudes. In fact, according to Mr. Maunder's diagram, in which different magnetic storms seem to be connected with different rotation-periods, we should expect just such a "band" in the periodogram of magnetic storms. Now Professor Schuster does *not* find this: he finds a well-defined bright line—the closely adjacent part of the spectrum being faint. I will now put on the screen a table representing the small portion of the spectrum or periodogram which he has examined. To bring out the facts clearly I have divided each number in his Table I. by the "expectancy"; so that real periodicities are represented by numbers sensibly greater than unity—say 4 or 5, for a real result: numbers less than these being due to accidental causes.

Table I.

Period in days.	1.	2.	3.	4.	9.	Mean.
27·250	2·5	0·8	0·4	0·4	0·1	0·9
27·278	0·5	5·9	1·1	3·1	1·7	2·5
27·333	0·6	0·6	1·2	0·7	1·7	0·9
27·542	3·7	0·3	0·4	0·8	0·2	1·1
Lunar	1·2	1·6	0·4	...	...	1·1

Under 1 is given the ordinate for the whole period given in the first column; under 2 that for the half-period; under 4 that for the quarter-period; and so on; these ordinates representing the harmonics of the main tone. It will be seen that it is the period of 27·278 days which shows "brightest"; not, however, the complete period, but the half of it. Professor Schuster finds the interpretation of the table "somewhat perplexing."

"We have, in fact, the choice between two interpretations:—

"1. Magnetic storms are apt to occur at times which, starting from a certain point, are multiples of 13·64 days. During some years the odd multiples, and during other years the even intervals, are principally concerned.

"2. Magnetic storms often recur after several successive intervals which are equal to some lapse of time sufficiently near 27·28 days to fall within the limits of rotation of the sun-spot zones."

I trust I have said enough to awaken interest in the paper and in the method of the periodogram generally; and I hope that the paper itself may be carefully read for the elucidation of other points necessarily omitted in this brief account—especially for the explanation of the use of the method in the present case, to which it may not seem at first sight to be strictly applicable.

Prof. Turner again occupied the Chair.

Prof. Larmor. I came here, Sir, on the invitation of Mr. Dyson, not to make a contribution but to listen. I am one of the people who have the pleasure of receiving the 'Monthly Notices' of this Society, and I was very much struck by the paper which appeared in the November number—so much so that I looked at it, not once, but day by day for about a whole week, I think. The particular feature in it—which, I think, is the statistics—amply satisfied me that these magnetic storms somehow or other occur in a period which is nearly the same as the rotation of the Sun, and I think an inspection of the diagram throws more light on the matter than all the tables that could be invented. Prof. Schuster, by his refined statistical method—which, by the way, Lord Rayleigh describes as one of the most notable advances in recent times in the discussion of statistics—points out, I understand, that the period in which these storms have a tendency to recur can be discriminated even to the small difference that there is between the lunar month and the period of the sun-spot rotation. I do not know how many days that is, or what fraction of a day it is.

A Fellow. Two days.

Prof. Larmor. Two days in twenty-eight.

The President. I think he has started out on a term which is one-tenth of a day.

Prof. Larmor. I thought he had arrived at something smaller than two days, and I can only express my astonishment that he is able to go to one-tenth with the amount of data available; in fact I am surprised. In looking at Mr. Maunder's diagram, which beyond any doubt shows that there is something which occurs in a period like the period which he has got there, or within half a day of it, I rather wondered whether the other statement he made, that the sun-spots affected particular meridians in the material of the Sun, was borne out. That would be a fact of extreme importance, even of more importance than the periodicity. I could not make myself certain that the magnetic storms which form the data fixed themselves up in certain meridian regions to the exclusion of others, but I notice that if, instead of dividing the diagram down vertically by lines, you divide it obliquely by lines, it is possible to separate them into groups much better that way; that, in fact, if lines are drawn obliquely across, so that they represent from top to bottom half a sun-spot period, I think there would result a division into groups. At first I wondered whether that was real. If it were real it would mean that the meridians most affected by sun-spots themselves had a rotation-period in the Sun of about 5½ years. It would seem, if that sort of thing could be established, that the cause of magnetic storms rotates in the Sun somehow as the Earth's magnetism rotates in the Earth in a period of thousands of years. But these oblique lines can be made vertical by altering the period, say, from 27⅓ to 27⅔ days, and I put the point in that way to suggest that Mr. Maunder might alter his diagram by slightly altering his period, and make a better case for himself as to their grouping in certain meridians than the present diagram makes. Of course, the essence of the whole affair is that the Sun has no particular period of revolution, but that one period belongs to the equator and another belongs to a little way up, and that, presumably, by taking as a period a third of a day more, he would get one that fitted better with a great majority of the sun-spots which are north or south of the equator and not on the equator itself, and therefore would go round slower than the equator does.

I believe the point ought to be pushed further. If sun-spots were analyzed instead of magnetic storms, and there are thousands of them as data, they could be sorted out into different zones of latitude. I think it would be a very interesting thing if Mr. Maunder could show us diagrams like that one, each diagram classifying sun-spots which occur in the narrow zone of latitude where the period of revolution shown by the sun-spots was something fairly definite. I do not know whether it is fairly definite, whether it is definite enough for the purpose. I should much like to see what would come out of that diagram, whether we should see some oblique lines of aggregation of the spots, as you see in this diagram of magnetic storms.

I have been looking just now at Prof. Schuster's paper, and I have marked some of the points. The first thing that I notice here is the point which was brought forward prominently by Lord Kelvin in a Presidential Address to the Royal Society, that no direct magnetic effect, due to magnetism in the Sun, is at all great enough to produce the magnetic disturbance on the Earth. I believe people must have known that long before Lord Kelvin's remarks. As a matter of fact, there is a paper in the 'Philosophical Magazine' of January 1884—I looked it up the other day—when I took some interest in these matters, and the calculation that Dr. Schuster gives here is entered at the end of that paper. I had no notion that it was new then. I owe to that my knowledge of the fact that I had anticipated Lord Kelvin's remarks, and I am sure other people had done so. I am rather bold in mentioning it, for my arithmetic may be wrong in comparison with Dr. Schuster's arithmetic, but I point out that the suggestion can take date before Lord Kelvin's statement as regards this country. There is another matter I should like to put right as regards the history of this notion that these magnetic storms of large scale cannot possibly be due to direct magnetism in the Sun as a whole —that it would require the Sun to be magnetized much more strongly than you could magnetize any kind of steel. That brings up the crux of the whole question — What is the cause of magnetic storms? They must be connected with the Sun somehow.

It is in my recollection that at the time Lord Kelvin drew attention to this matter, Prof. Fitzgerald immediately struck out the suggestion that they were due to streams of electrified ions projected from the disturbed regions. I do not know who gets the credit of that now. Mr. Maunder puts it down to Prof. Arrhenius—to his paper at the Royal Society three or four months ago. Various people before Arrhenius debated about it, in fact, it was largely in the air, but, so far as I know, you cannot put it beyond the date I looked up yesterday—ten years ago. If you look in the index of Prof. Fitzgerald's Collected Papers, which I had the honour to collect, if you look under the head of "Corona" you find he states very distinctly the cathodal theory of the large disturbance of magnetism which is now a very popular one. The objection to the theory—I do not know whether he had any means of getting over it; he had not, to my knowledge, when I asked him—the objection to the cathodal theory, is this: if the Sun is sending out these negative ions, the Sun would get an increasingly positive charge, and finally the cathodic rays would cease. Arrhenius' contribution to the matter is a suggestion for getting over the difficulty—a very striking one. Whether it is practical is another matter, but it is a very ingenious one, quite on a par with his other ingenious contributions to physics. As you know, Arrhenius' view is that it is not cathodal rays which are shot out of the Sun, but positive electrified things big enough to be repelled by the Sun's radiation; that they are driven away by the Sun's radiation; that the particles evaporate until they are small enough to reduce this preponderating repulsion, when they are drawn back again into the Sun by electric attraction. I think that is rather artificial. Curiously enough, the paper which I have mentioned in the 'Philosophical Magazine' of January 1884 is very largely concerned in another matter, which also Dr. Schuster is calculating here. There is a point which he has not got here, viz. that the presence of the upper conducting atmosphere necessitates that the magnetic poles of the Earth be north and south, as a rotating conducting body screens off from transverse magnetic influence. Of course, for currents induced in such a rotating upper region of the atmosphere by the Earth's diurnal motion in an extraneous magnetic field, the energy must come from the Earth's rotation. But I think you must have exciting magnetic causes outside the Earth to operate, so far as one knows, in co-operation with the magnetic rotation. Mere rotation by itself cannot transform its energy into magnetism unless there is magnetic machinery to work that. I have remarked on Dr. Schuster's discussion, perhaps without sufficient study of it. I notice, finally, the reference he makes to his own discovery in 1887, that you can ionize a gas, which has been the starting-point of so much experimental knowledge both on these matters and on the nature of electricity.

Mr. Newall. I almost hesitate to add any remarks to the discussion, because I feel the matter has been put to us so well in Prof. Schuster's paper and by Prof. Larmor. I have been trying, I confess, to dispel the doubts I have about this period which Mr. Maunder maintains that he has found; and so I am extremely interested to learn that Prof. Schuster has applied his powerful periodogram method to investigating the matter. I rejoice to hear you refer to Prof. Schuster's periodogram papers. They are both of them rather difficult papers to follow, and one rather wishes that they could be re-written so that they would become more easily intelligible to those who do not follow the intricacies of the subject as easily as Prof. Schuster does. A point I should like to have assurance about would be how far the confining of attention to periods so close to the one which Mr. Maunder maintains he has found is really a fair test of the matter. Supposing one applied similar calculations to any other period differing from the suspected one by an amount very much greater than one or two per cent., would one find intensities exceeding expectancy in a similar manner?

As Prof. Larmor has pointed out, it is a most interesting thing to study the apparent re-occurrence of the magnetic storms along *oblique* lines on the diagram which Mr. Maunder has produced; but one must take into account that on that diagram the oblique line must be inclined to the vertical axis by about 45 degrees in order to represent a change-period of about 1 per cent., and one sees that when one comes to pick out on the diagram evidence of periods differing very much from that chosen by Mr. Maunder, then one deals with lines so nearly horizontal that the method becomes insufficiently sensitive. The whole interest of Mr. Maunder's paper rests, it seems to me, in that one point—is it the case that Mr. Maunder has established this period in the recurrence of magnetic storms? Everything else is of secondary importance. That is the point of the paper to be established, and one welcomes his method of dealing with the subject, because, for the moment, he leaves sun-spots out of account altogether. Reference has been made to cathodal streams and so on; and here another difficulty arises which I think Prof. Larmor has pointed to already, but which I may refer to in other terms. Even when we have a sharp beginning to a magnetic storm, the epoch of that sharp beginning seems to be the same over an enormously wide extent of the surface of the Earth. Lately, careful studies have been made—I cannot remember the name of the observer—relating to the simultaneity of the outburst of magnetic storms at different stations, and it appears that even when the stations are not confined to one hemisphere there is close agreement between the epochs at which isolated storms occur. This is a point which requires explanation; but supposing we take the cause of these storms to be anything of the nature of a secondary effect of the arrival of cathodal emission, should we not expect to find that the storm began (especially when it had a sharp beginning) at a time when the place at which the observation was made was turned towards the Sun. In order to test this point I have used the valuable table of epochs of great storms in Mr. Maunder's paper to arrange the storms with which he deals according to the hour of the day at which they occurred, and, confining my attention to the 72 storms in which the beginnings are recorded as sharp, I get certain numbers for each hour of the day. These are affected by uncertainties, because of the proximity of the epoch of certain storms to exact hours; so I have taken the mean of the number in three successive hours as the mean representative number of storms in the middle hour. This gives us smoothed figures, as follows:—2, 2, 3, 4, 3, 2, 2, 3, 3, 4, 2, 2, 4, 5, 6, 5, 4, 3, 3, 3, 3, 2, 3, 3. There is no marked sign of the arrival of anything when the face of the Earth is turned towards the source of these electrified projectiles. In the first quarter of the day we have 16 storms, in the second 12, in the third 27, and in the fourth 17, or, roughly speaking, 39 during daylight and 33 in the dark, so that there is no very obvious evidence there of the arrival of something which does not affect the whole Earth at the same time. But that question is not a simple one, though it is dealt with in the view that attributes a storm, whatever the sharp beginning of the storm may be, to the rotational energy of the Earth, and a possible secondary effect of streams from the Sun altering the conductivity of layers in the upper air, and so affecting the magnetic needles below. We must all agree with Prof. Schuster's remarks about Mr. Maunder's bringing forward the paper; he has given quite a renewed interest to the whole subject.

Father Cortie. Prof. Turner has missed my point, which was not to deny in all cases the reality of the sequences, but to show that either they are not associated with sun-spots or, if they are associated with sun-spots, that the mode of action by which magnetic storms are caused by sun-spots is not that supposed by Mr. Maunder.

Mr. Newall, having asked permission to make a few further remarks, spoke about the apparent arbitrariness of the choice of period made by Mr. Maunder, and he demurred to the statement that the probability of any disturbance being succeeded by one in the next rotation within 10° of the same solar longitude is about one in forty; for since this range of longitude is allowed by Mr. Maunder in either direction, the probability is one in eighteen. The matter might be illustrated thus:—Instead of regarding Mr. Maunder's diagram as being made by rolling a very long narrow piece of paper, on which the epochs of the 276 storms are marked in appropriate places, round a cylinder of arbitrary diameter like the thread of a screw of fine pitch, we may regard the record of storms as a long bookshelf full of books among which red books represent magnetic storms while books of all other colours indicate absence of storms. The problem is to consider how far it is fair to divide this long bookshelf into short shelves of equal length of an arbitrary value to be set under one another in a very tall bookcase, and then by coincidental recurrence of red books on any vertical lines drawn across successive shelves in the bookcase, to judge of the existence of a rhythmical arrangement of red books along the original long shelf with a period represented by the length of the short shelves. Mr. Newall stated that he had been amused to find, on applying the analogue of Mr. Maunder's longitude criterion, which should, roughly speaking, put between one and five red books in each short shelf of, say, twenty books, the strongest evidence of rhythmical arrangement of the red books in a bookcase in his own room. He did not wish to lay undue weight upon this illustration, but believed that the only way to deal with the matter was by some such systematic investigation such as Prof. Schuster had undertaken. Until he had seen the details of this paper, he was not at once ready to give up his doubt as to whether a periodic recurrence had been established, even halving Mr. Maunder's value of the period. It seemed to him that account must be taken of a wider range of possible periods.

Mr. Cowell. There is nothing very original in Prof. Schuster's analysis, except perhaps the very striking analogy with the spectroscope. Prof. Newcomb used practically the same method thirty years ago when he discovered the Jupiter evection term in the Moon's longitude, and the 'Monthly Notices' of last November contain a paper of my own with a similar analysis. It is the ordinary method.

The President. In a recent conversation on this subject Mr. Maunder expressed to me the hope that his paper might be "severely heckled." I think that I may say that some small contribution towards the fulfilment of that hope has been made, and we shall now listen with great pleasure to what Mr. Maunder has to say further on the subject.

Mr. Maunder. I fear I cannot attempt to deal with all the subjects that have been raised this evening, but I should like to remark upon one thing that Prof. Larmor said. Mrs. Maunder and myself commenced a good while ago—several months ago—a sorting out of the sun-spots in precisely the way which he has indicated; that is to say, by distributing them, according to latitude, on a series of diagrams similar to Plate I. of my paper. We were also at work on the rotation-period of the Sun as deduced from the sun-spots, and our hope at that time was that we should get both done for last November meeting. I took up this magnetic paper as a purely incidental thing, not expecting to get much out of it, but when it worked out so differently from what we had expected, the other papers naturally got shelved for the time being. I am in hopes of having one or both ready in March or April, and although one does not like to anticipate a paper still on the stocks, I may say that we did find what seemed to us to be some clear cases of a sun-spot disturbance travelling through an entire series of longitudes, and some twice through. Such a drift, of course, takes a good many years. I should say that the distribution of latitude we are adopting is one of 5 degree zones, and we are grouping the spots in two sets. The centre of one zone in one set will correspond with the limits of two zones in the other, so that each sun-spot will appear twice, and if it has a great drift in latitude it still will be followed up.

With regard to the rotation-period of any particular latitude, it is not the case that it is at all constant. The rotation-period indicated in any latitude by spot-groups varies considerably, or you may express the fact by saying that spots in all latitudes have large proper motions. I was very greatly indebted to the

President for a copy of Prof. Schuster's paper, which I received last night, so that I might be able to look through it before coming to this Meeting, and I was personally much indebted to the President for the very clear explanation he gave of Prof. Schuster's paper just now. In one particular point, I think Prof. Schuster has not done me quite justice—that is, in the last paragraph, where he speaks of my "somewhat boastful claim." I did not mean it quite in the sense Prof. Schuster takes it. Lord Kelvin in his speech to which I alluded—at any rate it so appeared on the surface, and has been widely so interpreted, whether it was quite what he meant or not—gave us the choice of only two possible alternatives: either the coincidences between sun-spots and magnetic storms were all and always purely fortuitous and accidental, or we must assume the magnetic energy put out by the Sun to be so much—an altogether incredible amount. Well, to my mind, what I have done is this: I have shown from the times at which magnetic disturbances occurred that that particular idea of the solar action as of a magnet sending out magnetic waves in all directions was not the one which they presented to us, and therefore the alternative Lord Kelvin presented to us was not the actual alternative we had given us in nature; and in that sense I do think I have answered what he himself had termed "the 50 years' problem." I was extremely gratified to find that Prof. Schuster confirmed the main point of the paper, that the magnetic storms are related to the synodic rotation-period, but it seemed to me that the actual case in question was scarcely one for the application of his form of analysis. For, if I am correct as to this relation, these magnetic storms—that is to say, the times when they commence—depend upon at least some five different considerations. One is the mean synodic rotation-period of the Sun; another the special synodic rotation-periods of particular latitudes; third, the times at which these particular areas of the Sun are active; another the longitudes of these areas; and still another, as to whether the direction of their line of action is such as to intercept the Earth; and consequently it seemed to me it was not a case in which such an analysis could be expected at all to give any indication of the strength of the evidence for the relation I have discovered or to find out that relation itself except under special circumstances. Now these special circumstances, it happens, do actually take place, for, as I showed in my paper, there is a predominance of cases in which disturbances corresponded to the longitude in action when the great storm of February 1892 occurred. There is also a less marked predominance nearly 180° distant from it. But this, however interesting and important, is quite accidental to the main relation. I have examined not only the 22 years represented in my paper, but several earlier years, and I can put the whole case thus:—If you take out the greatest disturbance for each rotation-period of the Sun, then you will find that every five disturbances will give at least one pair associated in longitude. Now, Mr. Newall's "books" are not a case in point at all, because he had five on one shelf, four on another, three on another, and so on. If he had only had one book on each shelf he would have found his sequences very few indeed; they would not have come out in anything like the way we see in the case of the disturbances.

Mr. Newall. My case corresponds almost with the maximum of magnetic storms and not the average.

Mr. Maunder. Exactly so; but that is just the time when the sequences are least marked; though if they were purely accidental they would necessarily be most numerous then, because the disturbances were most numerous. It is when the solar activity is declining, when the average is less than one disturbance to each rotation, that the sequences are most numerous and last the longest. At minimum, when you have only three or four disturbances above a certain intensity in the year, of course it is not so easy to pick out sequences, as they are necessarily intermittent. There was another point in Prof. Schuster's paper to which I wish to draw attention. He spoke of the maximum of disturbances at the equinoxes and the minimum at the solstices as showing that they are due to the Earth and not to external influences. I have distributed the disturbances with which I dealt in periods of 18 days, so as to bring the Sun's node into the centre of one period, and the solstice into the centre of the next, and you see we get a maximum, not at the equinox, but in the period before, and we get a remarkable and most sharply marked minimum just when the Sun's equator is on the centre of the disc.

I did not introduce this diagram in my paper, for I was not satisfied that I had sufficient material to form a complete judgment on that point, and I only bring it forward now to show that it is not by any means certain that the maximum which occurs near the equinox occurs actually at the equinox itself. I think the point ought to be more fully investigated before making any definite statement on the subject. It is at least possible, in the present state of our knowledge, that the annual inequality in the number of disturbances may be due to the presentation of the Sun's axis to the Earth; not of the Earth's axis to the Sun. With regard to Father Cortie's two points, the one as to my logic and the other as to my observations, I think I will leave the logic for his further consideration. I will disclaim having used the word "cause" in any metaphysical sense. I think if we accept the idea, that Prof. Schuster puts forth, that the energy of the storms is in the Earth, but is released by solar action, that is sufficient justification for using the word "cause." But I am not going to stand for the exact word. Metaphysics lie outside my province altogether. My one object was to show there was distinct evidence of a *connection* between disturbances of terrestrial magnetism and the Sun, and I really think I have made out a very fair case for it.

The President. I am sure the writers of other papers who have been unfortunately disappointed in reading them will forgive us for crowding them out this time, because we have had a most interesting discussion on these solar matters.

The following papers were announced and partly read:—

Simon Newcomb. "The Eclipse of Agathocles."

E. Nevill. "On Terms of Long Period in the complete Expression for the Moon's Longitude."

P. H. Cowell. "The Longitude of the Moon's Perigee."

J. E. Gore. "On the Relative Brightness of Stars."

A. S. Williams. "On the Variable Star Y Aurigæ."

W. S. Franks. "The Spiral Nebula ♅ I. 153 Ceti."

A. Schuster. "Sun-spots and Magnetic Storms."

Rev. A. L. Cortie. "Magnetic Storms and Associated Sun-spots."

H. C. Plummer. "On the possible Effect of Radiation on the Motion of Comets."

Royal Observatory, Greenwich. "Note on the Re-determination of the Paris—Greenwich Longitude." (Communicated by the *Astronomer Royal.*)

A. Fowler. "Observations of the Spectra of Sun-spots, Region C to D."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon in the Year 1904." (Communicated by the *Astronomer Royal.*)

H. H. Turner. "Further Note on the Origin of Magnitude Equation in Photographic Measures."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Jupiter, 1905–6."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Saturn, 1905–6–7."

The following gentlemen were elected Fellows of the Society:—

Alexander John Samuel Adams, Post-Office Telegraphs, London Central, E.C.

Capt. Arthur ffolliott Garrett, R.E., Craigbeg, Kingussie, Scotland.

P. Groves Showell, L.C.C. School for Marine Engineering, Poplar, E.

George Bruce Halstead, M.A., Ph.D., Kenyon College, Gambier, Ohio, U.S.A.

William T. Litton, 'Shaftesbury' Training Ship.

Alfred Noël Neate, 49 Fulwood Road, Aigburth, Liverpool.

Alexander Durie Russell, B.Sc., High School, Falkirk, Scotland.

John James Steward, F.R.Met.Soc., 457 West Strand, W.C.

Lewis H. Tamplin, F.R.Met.Soc., Indo-China Steam Navigation Co., Wuhu, China.

David. Wylie, 9 East Road, Lancaster.

The following Candidates were proposed for election as Fellows of the Society:—

Brandon T. Brierly, F.G.S., Assoc.Mem.Inst.C.E., Linthwaite, Delph, Yorks (proposed by G. Calver).

Maurice Farman, Observatoire de Chevreuse à Jagny, par Dampierre (Seine-et-Oise), France (proposed by Camille Flammarion).

Willie Venner Merrifield, B.A., Head-Master, Nautical College, Byrom Street, Liverpool (proposed by W. H. Besant).

Isaac Molloy, M.A., Lützen, Glenageary, Kingston, Dublin (proposed by Louis G. Macrory).

Alfred Edward Nicholls, Principal of King Edward VII. Nautical School, London, E. (proposed by Sir Howard Grubb).

John Wearing, Garsdale, Sedbergh, Yorks (proposed by Thomas Weir).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1904 December 28.

President: A. C. D. Crommelin, B.A., in the Chair.

Secretaries: J. A. Hardcastle.
J. G. Petrie.

The Minutes of the previous Meeting were read and confirmed, and the list of presents received was read and the thanks of the Meeting accorded to the donors. The names of six Candidates for Membership were read and passed for suspension, and the election of one new Member by the Council was confirmed.

The President said that, before the ordinary business commenced, he wished to refer to the loss the Association had sustained in the death of the Rev. J. M. Bacon. Mr. Bacon, he said, was for several years a Member of the Council, and had taken a leading part in three solar-eclipse expeditions promoted by the Association. The Council had, that afternoon, passed a vote of sympathy and condolence with his family, with which he was sure the Members of the Association would be glad of an opportunity of associating themselves.

A paper by *Miss M. A. Orr*, on "Variable Stars of Long Period," was read by *Mr. Petrie*. The writer pointed out that while short-period variables ran through their changes with clock-like regularity, and could be explained by the eclipse theory and were all probably due to orbital or, in some cases, axial revolution, variables of the Myra type baffled calculation. There was a period, but the star could never be trusted to be accurate either in time or brightness. In many Mira variables also there was a distinct change of colour during the phase, and also of physical appearance. Was it possible that the variability of the Mira type was an exaggerated form of sun-spots? Dr. Johnstone Stoney had suggested that variation might become specially marked when the spot-period, supposing it to exist more or less in all stars, happened to coincide with what he called the "natural pulsation" of the star.

Mr. Maunder said Miss Orr's paper was a most suggestive one, but with regard to the main portion of it—the explanation of the long-period variables—he had always felt very great difficulty in accepting the analogy which had been sometimes drawn between the long-period variables and the variation of the sun-spot area of our own Sun. Not only was there the difficulty to which Miss Orr had alluded—the difference in period, the 11-year period being so very much longer than that of the long-period variables,—but there was also the difficulty of the scale of variation. In the case of the Sun the greatest amount of variation was perfectly inconsiderable, but in the case of Mira, for instance, and many other variables, there was a variation of from one to a thousand. The difference therefore seemed to be not only one of scale, but one of kind.

Mr. H. P. Hollis mentioned an hypothesis suggested by Mr. J. R. Sutton, which assumed the existence of several meteor-streams surrounding the star which periodically collided, the result being an explosion and an apparent increase of magnitude.

The President thought that Miss Orr had presented to them the great difficulty of the problem. It was certainly remarkable that some of the peculiarities in the sun-spot curve were reproduced in many of the variable stars, which seemed to suggest some analogy, although, as Miss Orr had shown, it could not be very close.

Dr. A. M. W. Downing read a paper on "The Date of Easter in 1905," explaining the apparent discrepancy between the almanacs and the ecclesiastical calendar as to the proper date for Easter in 1905.

Mr. Lynn said he did not think it was commonly known that the reason the 21st March was taken as the vernal equinox from which the full Moon which ruled the fixing of Easter was calculated, was so taken because that was its date at the time of the Council of Nicæa, A.D. 325.

The President said that those who drew up the tables given in the Prayer Book and elsewhere were well advised in keeping clear of the actual Moon of the heavens. The lunar tables were changed from time to time, and a very few minutes before or after midnight might, with new tables, make the difference of a day, besides which there would be the international difficulty caused by the change of meridian. It was very seldom that there was the difference of a month as there was in 1905; the difference of a week was comparatively frequent.

Father Cortie, in presenting the Report of the Solar Section for 1901, said it was drawn up in a similar form to those of the last few years, and aimed rather at giving the detailed life-histories of the spot-groups by means of a series of types than to give the areas and positions of the groups. The present report was interesting for two reasons: it dealt with a year of minimum solar activity, and it was the eleventh successive report issued by the Solar Section, so that the observations of the Section now covered a complete solar cycle. Referring to Mr. Maunder's paper read at the previous Meeting, Father Cortie said that it did not appear to him that the magnetic disturbances recorded at Stonyhurst altogether confirmed Mr. Maunder's views. Taking the year 1903, the biggest storm of the whole series was not connected with the great spot of October; this was accompanied by only a moderate storm. It was evidently not necessarily the biggest spots that were accompanied by the biggest magnetic movements. In 1901 they had the biggest storm of the year without a corresponding spot, and the biggest spot of the year without a corresponding storm during its period of greatest activity. When the spot came round again there was a magnetic storm. On the theory of a stream of particles issuing from a restricted magnetic area of the Sun, it was difficult to see why it should hit the Earth at the second shot and not also at the first. The last three storms of the series, those of August 14–15, September 10, and October 8, were separated by intervals that were approximately that of a synodical solar rotation, but there was absolutely nothing on the Sun with which to connect the leader of the sequence. Finally, there was no physical basis for the supposition that streams of particles were ejected from restricted areas of the Sun. To substitute streams of electrified particles instead of radiation from the Sun was a mere hypothesis. The rod-like streamers seen in some solar coronæ were not sufficient warranty for such an hypothesis.

The President said they were greatly indebted to Father Cortie for his Report. It was very satisfactory to see that, even if there had been no other observers in the world except their own Solar Section, all the more salient features would still have been preserved by the observations of the Section alone. Father Cortie's criticisms of Mr. Maunder's paper would be welcome as tending to throw more light on the whole subject. From the more points of view it was studied, the more likely they were to get at the truth.

A paper by *Mr. G. H. Lepper*, on "Modern Views of Markings on Mars," was next read. The writer briefly reviewed the various current theories on the subject, contrasting those of Lowell with those of Maunder and Evans, and maintained that the polar snow-caps afforded convincing proof that other bodies of water existed on the planet. He argued that the canaliform markings and "oases" ought to effectually dispose of the desert theory, and suggested that the theory of a ruddy vegetation, such as the South-African veldt in the dry season exhibits, best met the facts of the case.

Mr. Maunder said there were some things which they did know about Mars which ought not to be left out of sight in any discussion. The force of gravity at the surface of Mars was only about one-third of that on the Earth. Then it was pretty certain that the atmosphere was not nearly so dense as ours, especially at the surface of the planet, and its currents would therefore be much more gentle than here. Altogether the conditions were distinctly unfavourable to the existence of any form of life whatsoever upon the planet.

In reply to questions, Mr. Maunder said that Prof. Poynting had estimated the average temperature of Mars at −30° C. We saw the planet mostly at its noonday in summer time, and best in its tropical regions, and consequently were apt to be misled by what we saw.

Notes were read from *Mr. W. F. Denning* on "A very Low Perseid," and from *Miss C. O. Stevens* on "Halos and Rainbows."

Mr. S. A. Saunder then took the Chair while *The President* gave a *résumé* of two papers which he had written, the first on "The coming Spanish Eclipse," and the other "A simple Jovian Orrery." The first paper gave in tabular form the weather statistics of the most suitable points of observation in Spain, at the same time advocating the occupation of Palma, in Majorca, by at any rate some observers.

Mr. Saunder, in thanking the President on behalf of the Meeting for his valuable papers, urged the importance of all Members who intended joining one of the expeditions sending in their names as soon as possible.

ROYAL METEOROLOGICAL SOCIETY.

The Annual General Meeting of this Society was held on Wednesday evening, January 18, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker, F.R.S.E.*, President, being in the Chair.

The Council, in their Report, referred to the honour which H.R.H. the Prince of Wales had conferred upon the Society by graciously consenting to become its Patron. The work in connection with the exploration of the upper atmosphere had been continued. During the summer the Admiralty placed at the disposal of the Kite Committee H.M.S. 'Seahorse' for the purpose of carrying on the kite observations off Crinan under the direction of Mr. Dines. The average height attained was about one mile.

The President delivered an Address on "The Connection of Meteorology with other Sciences." He said that Meteorology and Astronomy were doubtless the first of the sciences to attract the attention of men; which of the two exerts most influence in the well-being of humanity is a matter dependent on one's position on the globe; in many regions people are but slightly affected by the weather, while the heavenly bodies, particularly the Sun, exert an enormous influence on human life. Everywhere in Nature we find the effects of meteorological agencies. After speaking upon the effects of evaporation, winds, rain, ice, snow, and pointing out the influence of weather on animal life, vegetation, health, &c., he said that Meteorology is a science deserving more attention than it receives. He thought it ought to be recognized as a preliminary to the studies of geography, geology, and kindred subjects, and he was of opinion that meteorological observatories might very well be fitted up in schools, and pupils taught to observe. This could be done at a small cost of time or money. The tendency at present is to particularize in all scientific work, but the true path to progress lies in keeping a comprehensive outlook on the whole field of investigation. The United States have devoted much attention to Meteorology with most satisfactory results. It is to be regretted that official help and encouragement is so deficient in this country. The baffling, difficult nature of meteorological problems should but serve as an incentive to their elucidation. The persistent observer gains much, not only in knowledge of the subject, but in the habits of close and accurate investigation which he insensibly acquires, and all workers in this field learn to appreciate the difficulties which confront their fellow-labourers, and to recognize the value of what has been done by the meteorological organizations of the world.

Mr. Richard Bentley, F.L.S., was elected President for the ensuing year.

Fireballs of 1904 *December* 18 *and* 22.

In the evening twilight on Sunday, December 18, $4^h \cdot 38^m$, and Thursday, December 22, $4^h\ 30^m$, very brilliant meteors were observed from various parts of the country. Unfortunately, however, the apparent paths were not very accurately recorded, only a few stars being visible at the time of their appearance, though Venus and Jupiter were shining conspicuously.

Five observations of the fireball of December 18 have reached me, while six observations of that of December 22 have come to hand.

Mr. H. Michell Whitley, of Eastbourne, writes me that on December 18, $4^h\ 40^m$, he saw a meteor equal to Venus passing from $290° + 27°$ to $273\frac{3}{4}° - 3°$ in 2 seconds. He could see no train, but before disappearance the nucleus burst into two, which shot downwards, one much in front of the other.

Mr. F. L. Raymond, of Yeovil, was observing Venus on December

18, and at 4^h 38^m saw a large meteor about 3 × ♃, with a flight obliquely descending from left to right, about midway between Jupiter and Venus; colour bright green. The meteor divided into two before extinction; no trail.

Mr. R. Callard, of Ammanford, Carmarthen, also observed the meteor and describes it as beautifully large and bright. It fell from about the centre of Pegasus, perpendicularly downwards, and occupied about 4 or 5 seconds; midway in its flight it divided into two, bearing a double train.

Capt. Clifford, A.D.C. to Gen. Sir Wm. Butler, Commanding Western District, was between Causand and Cremyll, near Plymouth, and saw a very fine meteor which formed a splendid sight just before sunset. "Its course was roughly from S.W. to N.E. Both Sun and Moon were visible at the time."

The same object was noted at Penarth and Carmarthen, but no details are given. I have been in correspondence with the various observers and obtained some further particulars, but the real path cannot be derived with accuracy. Probably the radiant was in Cepheus at about 337° +60°, and the approximate height of the object from 81 to 30 miles over the English Channel some 30 miles S. of Portland Bill. Path about 64 miles, and velocity 20 miles per second.

Fireball of December 22.—Mr. J. B. Cossins, of Tarporley, Cheshire, says that at 4^h 30^m he noticed a ball of fire descending through the air. It left a line of flame after it and burst when close to the ground. It came from a S.W. direction and fell obliquely from left to right.

A son of Dr. D. Smart, F.R.A.S., saw the meteor at Merthyr Tydvil, and descibes it as exceptionally bright though daylight was strong. The observed flight was vertically downwards in about N.E., from about 70° to 50° altitude. These were, however, only rough estimations, the observer being on horseback on the Aberdare Mountain, between Merthyr and Abernant, at the time, and the fog so thick that only the mountain tops visibly protruded.

Mr. Richard Myddelton, of Chirk Castle, North Wales, saw the meteor from the valley of Ceriog. It fell straight down and burst in the E.S.E. The meteor seemed very near and apparently exploded at a distance of some 500 or 600 yards, but the observer regards this as probably an optical illusion.

Mr. Francis Wrightson, of Chichester, says the meteor fell almost vertically in the N. with a slight inclination from W.

Mr. F. J. Garrard, of Louth, observed the meteor descending about 5° W. of Venus to near the horizon. It first caught his eye when at an elevation of 60°, and the flight occupied 1 to 2 seconds. "Its path, if extended upwards and backwards, would not exactly have crossed the zenith, but a point some 5° or 10° south of it, and most certainly did not pass N. of the zenith."

Mr. H. R. Marten noticed the meteor from Portsmouth Harbour. He was looking N.W. at the time. The object was green in colour and apparently so close that he thought it fell in Westfield.

It is difficult to deduce any exact result from these observations, but the meteor may have been directed from a radiant at about 330° + 36° and have fallen from 83 to 28 miles in height over the district from Wellingborough to Towcester.

The foregoing details have been given in the hope that further information may be elicited as to the observed paths of the pair of fine meteors alluded to. The circumstances were admittedly unfavourable, and the observers quite unprepared to obtain exact observations; but the weather was very clear and frosty, and it is quite possible that some additional data may be forthcoming, with regard to the two objects, which will lead to the more reliable determination of their real paths.

Bishopston, Bristol, W. F. DENNING.
1905, Jan. 15.

Early Suggestions of the Indication by Magnetic Disturbances of the Solar Rotation-Period.

WHEN I had completed my paper on the "Magnetic Disturbances, 1882–1903, and their Association with Sun-spots," communicated to the Royal Astronomical Society on 1904 November 11, the result of my research reminded me of the work of Dr. Ch. Hornstein some thirty years before, and whilst referring to it I was directed to a very suggestive paper by the late James Allan Broun, F.R.S., appearing in the 'Philosophical Transactions' of 1876. It will be within the recollection of most that Mr. Broun was the Director of Sir Thomas Makdougall Brisbane's Observatory at Makerstoun, and later of the Magnetic Observatory at Trevandrum, Travancore, India. In this paper Broun comes exceedingly near to demonstrating the tendency of magnetic disturbances to recur at intervals corresponding to the synodic rotation-period of the Sun. Indeed, as will be seen from the following quotations, several of his suggestions anticipate in a most striking manner the conclusions which I reached in my paper of November last. He failed, however, to completely establish his point, chiefly because he confined himself to the observations of two years, and, so far as I know, never extended his examination further. It is evident, therefore, that he himself did not regard his suggestion of this relation as more than a plausible one, and did not think it sufficiently important to call for a more complete enquiry. The reasons which led him thus to undervalue the result which he had already in his hand were probably three in number: he adopted an erroneous value for the mean rotation-period of the Sun; he did not realize the fact—or at any rate did not grasp its practical application—that the solar spots rotate in different periods; and, lastly, it seemed impossible for him to deal with his observations as they stood, without smoothing their significance away in hourly, daily, monthly, or annual means.

The following quotations from his paper on the "Variations of the Horizontal Force of the Earth's Magnetism" exhibit the paragraphs which are of most interest in the present connection. The table which I have taken from the paper I have slightly altered, in that I have given the duration of the disturbance instead of the time of the ending as well as that of the beginning. I have also computed the longitude of the Sun's centre, assuming the same prime meridian and rotation-period, as in the Greenwich Heliographic Results. The rotations I have numbered backwards from the Greenwich fundamental rotation, namely that commencing 1853 November 9, which is No. 1 in the Greenwich numeration.

Mr. J. A. Broun, "On the Variations of the Horizontal Force of the Earth's Magnetism." Phil. Trans. 1876.

P. 388.—Although an examination of the Trevandrum observations confirmed me in the belief that the Sun's rotation was the great cause of the variations of daily mean horizontal intensity, yet trials for the greatest mean oscillation, employing periods of a few tenths of a day more or less than 27·2 days (the time given then by the best authorities for the Sun's synodical rotation), were not satisfactory, and the more complete investigation was deferred till the discussion of a longer series might enable me to draw more certain conclusions. Meanwhile, having found that the observations made at Trevandrum in 1844 and 1845 showed almost exactly the same variations as at Makerstoun, I returned to the examination of the observations made during these two years, and deduced from them, and from the observations made simultaneously in different Colonial observatories, that the mean duration of the oscillation was 25.96 days. This period differed so much from that then accepted for the Sun's rotation that I suggested some movement, regular or irregular, of the Sun's magnetic poles or of the meridian of the maximum force *.

P. 389.—One of the most marked objections to the constancy of the solar action for given solar meridians for any considerable period was to be found in the middle of the series of well-marked oscillations for 1844. In the months of January to April there were four successive movements with periods of from 25 to 29 days, each having amplitudes of about one-thousandth of the whole horizontal force; in the months of May, June, and July the oscillation may be said to have disappeared, while in the following months the movement reappeared as distinctly as in the first months of the year. It is the principal object of the present communication to show that this apparently anomalous result is due to the joint actions of the Sun and Moon †.

* * * * * *

P. 398.—We have seen that when one side of the Sun is presented to the Earth the magnetic force of the latter is greater than when the other side is turned towards us; we may even say that the intensity is greatest for a given solar meridian; this, however, may be simply an integral effect resulting from the actions due to all the meridians. But can we suppose, when a great and sudden increase or diminution of the Earth's magnetic force occurs, that this is produced by some change occurring on a particular solar meridian? This does not seem at all improbable. It could scarcely, however, be supposed that the effect produced on the Earth's magnetism could be due to the given solar meridian being presented to (that is, in the same plane with) the Earth, unless we admit the idea that these solar magnetic actions are propagated only in certain definite directions, or that the Earth has some action in the production of the solar change when the given meridian is presented to it.

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* [In other words, Broun was realizing that the area of the Sun's surface giving rise to the disturbances was not always the same.—E. W. M.]

† [Had Broun known of the intermittent character of sun-spot activity, he would not have considered this result anomalous.—E. W. M.]

In order to examine the facts, all the cases were noted during the years 1844 and 1845 in which the *daily mean* horizontal force diminished one-thousandth of its whole value *within* an interval of three days; they were found to be twenty-eight in number.

* * * * * *

Cases in which the Earth's Magnetic Force (*i. e.* the daily mean horizontal force) diminished one-thousandth of its whole value or more in 1844 and 1845.

No. of case.	Date of beginning.	Duration.	Change of force in hundred-thousandths.	Rotation.	Longitude of Sun's centre.
		days.			°
1	1844 Mar. 28	2	−360	−128	296
2	Apr. 25	1	104	−127	287
3	May 21	2	107	−126	303
4	July 8	1	116	−125	27
5	Aug. 1	1	175	−124	70
6	9	1	135	−123	324
7	Sept. 24	3	115	−122	77
8	30	1	104	−121	357
9	Oct. 19	2	268	...	107
10	Nov. 19	2	130	−120	58
11	Dec. 27	3	165	−118	277
12	1845 Jan. 8	2	210	...	119
13	18	2	163	−117	348
14	Feb. 20	1	118	−116	264
15	Apr. 12	2	350	−114	321
16	June 10	2	110	−112	261
17	July 24	1	154	−111	39
18	31	3	102	−110	306
19	Aug. 26	1	101	−109	322
20	29	1	159	...	282
21	Sept. 24	1	153	−108	299
22	Oct. 8	2	126	...	114
23	20	1	118	−107	316
24	30	2	100	...	184
25	Nov. 15	3	100	−106	333
26	27	2	110	...	175
27	Dec. 3	1	291	...	96
28	11	2	122	−105	351

P. 400.—If any doubt existed as to the possibility of these being mere accidental coincidences, it would be removed, I think, by a consideration of the marked succession occurring between July 31st and December 11th, 1845. It will be observed that though considerable magnetic changes occur successively near the same solar meridian, yet that they do not always occur in successive rotations. . . . . For this reason the succession mentioned merits particular

> consideration. . . . . We have five cases of successive solar rotations in which the diminutions of intensity began on the +8 day. This exact recurrence at the end of 26 days of the marked diminution of force proves, it seems to me, *that the actions are all due to the Sun*, whose time of rotation must be very nearly 26 days.
>
> An examination of the projected means will show that the sudden diminutions of terrestrial magnetic force are in nearly every case preceded by a sudden increase. It seems extremely difficult to explain these regular pulsations, which are felt all over the Earth, by any theory of distribution of magnetism on the solar surface, or by any variations of temperature on the Earth, or to understand why the sudden increase should have ended and the rapid diminution of magnetic force should have begun exactly when the solar meridian +8 arrived at the same position *relatively to the Earth*, without admitting that the Earth itself had some part in their production—a part which might be due, however, merely to her passage through some ray-like, electrical emanation from the Sun.

Short as is the period over which Broun's catalogue extends, and rough as are the times given for the commencement of the disturbances—no hour being stated but only the day,—yet it brings out their tendency to recur in the solar rotation-period in a most striking way, as the following sequences will show. The longitudes have all been taken out for Greenwich noon, and hence must be subject to an uncertainty of several degrees; yet the longest sequence, as Broun points out, runs through five consecutive rotations with absolute smoothness. In all twenty-one out of the twenty-eight disturbances, extending over twenty-seven rotation-periods of the Sun, fall into one or other of these sequences; six of which are pairs, one is of four members, and one of five.

Reference No. of Disturb.	No. of Rotation.	Longitude of Sun's centre.	Reference No. of Disturb.	No. of Rotation.	Longitude of Sun's centre.
		°			°
1	−128	296	18	−110	306
2	−127	287	19	−109	322
3	−126	303			
6	−123	324	20	−109	282
			21	−108	299
5	−124	70	23	−107	316
7	−122	77	25	−106	333
			28	−105	351
9	−121	107			
12	−118	119	22	−108	114
			27	−106	96
14	−116	264			
16	−112	261	24	−107	184
			26	−106	175

Another research, dimly brought to my recollection, was one by the late Prof. Ad. Quetelet. I have not been able to identify the reference which was in my mind, and possibly may have been mistaken in ascribing it to Quetelet; but the research in question brought out that displays of auroræ tended to recur at intervals of twenty-seven or twenty-eight days. Quetelet's papers afford a good many instances of great auroræ recurring at these intervals, but, as I have just said, I have not been able to verify my impression that he associated these with the solar rotation-period.

But whether he did or not, it is perfectly certain that Dr. M. A. Veeder has done so, and asserted this relation as long ago as 1893 (*American Meteorological Journal*, vol. ix., June 1893). I had known of Veeder's conclusions with respect to auroræ, but for two reasons had entirely failed to give them the consideration they deserved. First, it seemed to me possible that the period of 27⅓ days which he had found might be apparent only, and due to the fact that the distinctness of auroræ depends to some extent upon the absence of the Moon. The other point was that Veeder has always insisted upon the connection of auroræ with spots upon the east limb of the Sun, a relation which my own observations did not enable me to confirm, and I believed to be mistaken. I had therefore allowed his research to pass out of my mind.

My own research was entirely independent of any previous worker in this field, and the line which I followed was special to myself; but it is only right that Broun and Veeder should receive due credit for their earlier results, and it is for that purpose that I have written the above note. E. Walter Maunder.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The First Director of the Sydney Observatory.

Gentlemen,—

Two important errors occur in Mr. Lynn's interesting letter on the above subject in the *Observatory* for October last. It is stated that a new transit-circle arrived from England, and that observations were commenced with it in 1859. This is a mistake. The transit-circle employed throughout by Mr. Scott was no other than the old Parramatta instrument, which had been sent to England for repairs. Even after repairs it was not an instrument adapted to the requirements of a modern national observatory. Mr. Scott encountered great difficulty in securing proper instruments for the new establishment under his direction. Were it not for this difficulty Sydney would now be able to boast of a Catalogue of Southern Stars equal to that which has made southern observers so grateful to Mr. Stone. It was Mr. Scott's ambition to re-observe all the stars of Lacaille's well-known Catalogue, but in consequence of the short-sightedness of those in authority, his object could not be attained. It was not till 1877, under the directorship of Mr. Russell, that the Sydney Observatory was furnished with a new and excellent transit-circle. Another and more important error is that Mr. Scott is stated to have died at Sydney in the year 1889. I do not know how this error could have arisen. I saw my old friend at his home on the Kurrajong Mountains about three years ago, and I believe he is still alive and well. At all events his name occurs in the list of living members of our local Royal Society for 1903. Yours faithfully,

John Tebbutt.

Observatory, Peninsula,
Windsor, N. S. Wales,
1904, Nov. 21.

[Mr. Lynn asks us to state, with regard to the first part of the above letter, that he quoted from the first volume of the Sydney Observations (Introduction, p. vi), "The transit-circle arrived from England about the end of December 1858, but the regular observations were not commenced until June 1859." This does, however, relate, as Mr. Tebbutt says, and as appears from the second volume, to the Parramatta transit-circle (by Jones), but it had been sent to England, where it was repaired, improved, and re-divided by Troughton and Simms. Before seeing Mr. Tebbutt's letter, Mr. Lynn had himself received one from Mr. Scott (dated December 5, 1904), in which he says that though, from a breakdown in health, he retired from active life in 1882, he is still living and "in fairly good health" at Kurrajong Heights, a well-known health-resort in Australia, about fifty miles from Sydney, on a spur of the Blue Mountain Range. Mr. Scott is now in the eightieth year of his age, and we hope will yet live many years to continue to take an interest in our science.—Eds.]

The Cluster in Cancer.

Gentlemen,—

A very singular error has been made in the ordinary rendering of the name of this cluster (or nebula, as it seems to the naked eye) into English, and is repeated in many astronomical works, the first (so far as I am aware) being the 'Treatise on Astronomy' of Sir J. Herschel (1833), which was afterwards enlarged into the 'Outlines.' The cluster in question is mentioned in the 'Prognostics' of Aratus, who calls the two stars now known as β and γ Cancri, ὄνοι (asses), and speaks of a luminous appearance between them, which he calls φάτνη, *i. e.* manger. Pliny (Nat. Hist. xviii. 80) calls the stars "aselli" and the cluster "præsepia," the plural of "præsepe." Now "præsepe" in Latin may mean a bee-hive, and is so used in one place in Vergil's 'Georgics' (iv. 168), "ignavum, fucos, pecus a præsepibus arcent," *i. e.* "they drive off the drones, lazy cattle, from the hives." But its original sense is an enclosure of any kind, and it is also used to denote a manger, as in Phædrus (lib. ii. fab. viii. 22), "accidit ad præsepe," *i. e.* "he [the master] approaches the manger." In this sense undoubtedly Pliny means to take it, translating the Greek φάτνη, which *can* only signify a manger. Indeed, foolish as we may think asses, they would not be likely to plant themselves on either side of a bee-hive.

Yours faithfully,

Blackheath, 1904, Nov. 15. W. T. Lynn.

P.S.—Theophrastus, I may remark, speaks (De Signis Tempestatum, 51) of only one ὄνος, or ass, and his φάτνη, or manger. Probably this refers to δ Cancri, which is somewhat brighter than γ.

Periodical Comets due in 1905.

Gentlemen,—

Mr. Lynn, in his notes on the above, omits all reference to Tempel's Comet of 1867, which is due to return to perihelion in 1905 April. This comet has been subject to considerable perturbation by the planet Jupiter. It was seen at its returns in 1873 and 1879, but has not been re-observed since the latter year, though it must have returned in 1885, 1892, and 1898. The period was formerly 5·982 years, but this has been increased by the action of Jupiter to 6·539 years, and the perihelion distance, which was 1·56, is now 2·07 (see Gautier's elements in *Ast. Nach.* 3492, and *Observatory*, vol. xxi. p. 243).

As to Wolf's Comet of 1884, it is also due at perihelion in 1905 April, but the conditions will be very unfavourable, the Earth and comet being on opposite sides of the Sun.

Barnard's Comet of 1892, which was a faint object discovered by photography, may also return near the close of 1905, as its period is probably about 6½ years. At its last return, in 1899, nothing was seen of the comet, and its exact period being doubtful, it may again escape detection. Its re-discovery at a future time will more probably be effected by some comet-hunter who will alight upon it in the ordinary progress of his work.

You mention (*Observatory*, January, p. 67) Giacobini's cometary discovery on December 17 last as his seventh, but it seems to have been his eighth, he having detected new comets as under:—

1896 September 4.
1898 June 18.
1899 September 30.
1900 January 31.
1900 December 20.
1902 December 2.
1903 January 15.
1904 December 17.

Yours faithfully,

Bristol, 1905, Jan. 7. W. F. Denning.

On Dr. de Sitter's "Remarkable Error in Damoiseau's Tables."

GENTLEMEN,—

The discrepancies brought to light by Dr. de Sitter in the last number of the *Observatory*, between Damoiseau's positions for the nodes of Jupiter's Satellites and those of Delambre, as given by Laplace in the 'Mécanique Céleste,' have a simpler explanation than the one which he supplies. The node is denoted by Laplace, not by Λ, but by $-\Lambda$. Hence the signs of the numbers given by Dr. de Sitter should be reversed. Besides this, the value given in the 'Mécanique Céleste' for the node of Satellite II., viz. $-303^{\circ}\cdot 8$ centesimal, is a known error, corrected to $-285^{\circ}\cdot 2$ by Delambre as long ago as 1817, in the Introduction to his Tables. When these corrections are made, we have, not the values given by Dr. de Sitter,

	Sat. II.	Sat. III.	Sat. IV.
	93°	7°	255°
but	283°	353°	105°
to compare with Damoiseau's values,	282°	353°	98°

I need not follow Dr. de Sitter's further comments, as these changes throw them into some confusion, but it should be remarked that the statement that the eclipses give no criterion to distinguish the ascending from the descending node is an error. A moment's consideration of the expressions for the latitude of the satellites as they enter the eclipses, and of the physical circumstances which these expressions imply, will refute it.

University of Durham Observatory,
1905, Jan. 7.

Yours very truly,
R. A. SAMPSON.

Lunar Rainbows.

GENTLEMEN,—

With reference to the Rev. S. J. Johnson's account of a lunar rainbow in your number for this month, perhaps you can find room for two notes I made in the year 1900, viz.:—

"1900 Oct. 4th. Beautiful double lunar rainbow; primary bow complete, secondary bow wanted a very small portion at the eastern end. Colours well shown in the primary, but barely discernible in the secondary bow. Projected on an *apparently* clear sky; could see θ and ψ Ursæ Majoris shining brightly through the inner bow. Lasted from 9.10 to 9.30 P.M."

"1900 Dec. 11th. Another remarkable lunar rainbow; very bright; colours well seen in the primary bow, which lasted *continuously* from 10.55 P.M. to 12.40 and perhaps longer; a most unusual duration; faint traces only of secondary bow."

I was watching for Geminids on this latter occasion, the eastern sky remaining clear, while in the W. and N.W. a fine Scotch mist was coming off the hills. Is there any record of a longer duration?

Yours faithfully,
CHARLES L. BROOK.

Harewood Lodge, Meltham,
1905, Jan. 12.

PUBLICATIONS.

AN ASTRONOMICAL GUIDE *.—At first sight we feel inclined to object to the title of Sir Robert Ball's new book. The word "Guide"—reminiscent of Bradshaw—seems scarcely dignified enough to be applied to such a portly and handsome volume. This is actually a second edition of the same author's 'Atlas of Astronomy,' published in 1892, whose title somehow appears to be more suitable. The contents of the new work are named in the first paragraph of the Preface—"a summary of our present knowledge of the solar system, a guide to the positions of the planets for the first half of the present century, a series of star-maps, some examples of the finest achievements in the art of drawing and photographing celestial objects, and a list of interesting objects which may be observed with small telescopes." The examples of photography—which is one of the points in which this excels the earlier book—include specimens selected by Mr. Hinks, whose help the author gratefully acknowledges, from the collections at the Yerkes, Lick, and Paris Observatories, from those taken by the late Dr. Roberts, by Dr. W. E. Wilson, of Daramona, Westmeath, and by Mr. Stanley Williams, as well as photographs from Greenwich and the Cape.

Among other improvements it may be noted that the ground of the star-maps is now coloured of a light blue tint, pleasing to the eye, which seems to show up the stars more distinctly; and in connection with these maps the ingenious tables should be mentioned which, as the above quotation from the Preface implies, give a ready means of finding the position of a planet in any month of the years from 1900 to 1950. These "Indexes to the Planets" were in the 'Atlas,' but among the noteworthy additions one that strikes us as being useful is Diagram No. 1, which shows the Earth in its orbit, the whole surrounded by the celestial sphere—a configuration that one often has to make mentally or on paper.

It may be put down to prejudice, but we fancy that the plate of types of the corona published in this magazine some years ago, after M. Hansky, would have been more effective than Plate 16, and the colouring of the Moon pictures seems to our eye somewhat crude; but these small details cannot prevent us from recommending anyone who has occasion to use an astronomical reference book to buy this, and anyone who does not want a reference book to buy it for its pictures.

* 'A Popular Guide to the Heavens; a Series of 83 Plates with Explanatory Text and Index.' By Sir Robert Stawell Ball, LL.D., F.R.S. London: George Philip & Son, 1905. Price 15s.

NOTES.

Comet Notes.—Brooks' Comet of last April (1904 I.) was photographed at Greenwich in the middle of January. It showed quite a strong image, and was probably not fainter than the 11th magnitude. An ephemeris (for Greenwich midnight) follows; it required corrections of -5^{s}, $-0'{\cdot}8$ on Dec. 7.

An ephemeris is also given of Tempel's Comet (*c* 1904) (for Paris midnight). This, however, is very faint and low down in the evening twilight.

Date.	Brooks. R.A.		N. Dec.	Tempel. R.A.	S. Dec.
	h m s		° ′	h m s	° ′
Feb. 5	10 39 49		64 2	23 21 34	12 29
11	10 17 53		64 30	23 38 5	10 55
17	9 55 56		64 37	23 54 6	9 21
23	9 34 55		64 25	0 9 37	7 48
Mar. 1	9 15 34		63 55	0 24 41	6 17
7	8 58 27		63 11	0 39 21	4 47

The following elements of Comet *d* 1904 (Giacobini) are by Martin Ebell, from observations on Dec. 17, 21, 26:—

T........ 1904 Nov. 4·22 Berlin M.T.
ω........ 41° 16′
☊ ...... 218 32 } 1904·0.
i........ 99 39
log *q* .... 0·2754

The following elements of Comet *e* 1904 (Borrelly) are by Elis Strömgren, from observations on Dec. 31, Jan. 1, 2:—

T........ 1905 Jan. 1·27 Berlin M.T.
ω........ 341° 23′
☊ ...... 69 55 } 1905·0.
i........ 35 31
log *q* .... 0·1934

The following rough ephemerides of the two comets are for Berlin midnight. The Borrelly ephemeris has been corrected by observations on Jan. 10 and 25. It was then very faint (about 11th magnitude), and the brightness is steadily diminishing:—

	Giacobini. R.A.	N. Dec.	Borrelly. R.A.	N. Dec.
	h m s	° ′	h m s	° ′
Feb. 1	18 46 10	51 9	2 11 47	16 7
5	19 3 58	53 3	2 20 34	18 47
9	19 22 36	54 51	2 29 42	21 14
13	19 42 4	56 33	2 39 17	23 38
17	20 2 0	58 0	2 49 11	25 54

A. C. D. C.

MINOR PLANET NOTES.—The following planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Mag.
PN	1904 Dec. 14	Wolf.	13·2
PO	Nov. 16	„	12·5
PP	Dec. 14	„	13·0
PQ	1905 Jan. 1	„	13·2
PR	1	„	12·8
PS	8	Götz.	10·0
PT	14	Wolf.	13·0
PU	14	Götz.	12·5
PV	22	Wolf.	13·5

A planet discovered at Washington on Dec. 30 is probably 415 Palatia. The opposition data of this planet in the *Berliner Jahrbuch* appear to be erroneous. PU may be 489.

The following planets have been named:—509 Jolanda, 512 Taurinensis, 516 Amherstia, 521 Brixia, NY Herculina.

The planet PV deserves special mention, as this planet was in the neighbourhood of Jupiter when discovered, and was evidently the result of a search for the recently discovered satellite. It was 75′ east of Jupiter, 43′ north, and these quantities were increasing 15′ and 6′ daily respectively. Hence this object cannot be a satellite of Jupiter. Prof. Wolf considered that this was the same object as that announced as a satellite at the beginning of January. This conclusion, however, seems improbable, as Prof. Perrine observed the satellite on Jan. 17, 36′ west of Jupiter. A. C. D. C.

OBITUARY.—PAUL HENRY. It is with deep regret that we have to chronicle the death from cerebral congestion of this valued member of the staff of the National Observatory, Paris. M. Paul Henry was suddenly seized with an apoplectic fit on January 3, and survived but a few moments. It will be remembered that his brother, M. Prosper Henry, died suddenly as the consequence of an accident on 1903 July 25, and the short note we published at that time of his life and work might be again given here as applying to the gentleman just dead, for the Brothers Henry worked together through life, so that in the minds of most persons they were seldom dissociated. Beginning life as working opticians at Nancy, they came to Paris in 1864, and their services were secured by M. Le Verrier for the Paris Observatory. They discovered between them 14 small planets, and did some photographic work. To them is largely due the inception and realization of the Astrographic Chart. M. Paul Henry was nearly 55 years old at the time of his death.

EDWARD CROSSLEY. Announcement is also made of the death of this gentleman, whose name is kept in memory at the Lick Observatory by being applied to the 3-foot reflecting telescope.

Mr. Crossley was the son of the late Mr. John Crossley, of Halifax, the head of a large firm of carpet manufacturers, to which position the son succeeded. Mr. Edward Crossley was three times mayor of the borough of Halifax and represented the Sowerby division of West Riding in Parliament, as a Liberal member, from 1885 to 1892. But besides these occupations Mr. Crossley was an amateur astronomer. He was elected a Fellow of the Royal Astronomical Society on 1867, June 14, and soon after set up an observatory, the principal instruments of which were a 9⅓-inch equatorial refractor by Cooke and a transit-circle of 3½ feet focus with declination circle 18 inches in diameter; here, with Mr. Joseph Gledhill, he observed double stars, phenomena of Jupiter's satellites, and occultations of stars by the Moon, all of which are noted in the annual report of the observatory which has been included year by year in the February number of the 'Monthly Notices.' They also collected notes on double stars from other sources, and Crossley and Gledhill's 'Handbook of Double Stars' is a standard work. In 1885 Mr. Crossley bought from the late Dr. Common the 3-foot reflector with which he had taken his celebrated picture of the Orion Nebula, and several papers will be found in the 'Monthly Notices' relating to the adjustment of this instrument. After keeping this for ten years he presented it to the Lick Observatory, and though, as has been said, the instrument has undergone such alteration that little of the original remains, still it retains the name of the donor, and it is a little remarkable that such an achievement as the discovery of a new satellite should have been made with it just before his death. Mr. Crossley died suddenly from heart failure on the morning of Saturday, January 21, in the 64th year of his age.

ERNST ABBE. The obituary roll for the past month also includes the name of Dr. Abbe, the head of the famous optical firm of Carl Zeiss at Jena. Abbe, born on January 23, 1840, was the son of a foreman in a spinning-mill, and, after taking his degree at Göttingen, devoted himself to the study and teaching of physics. In 1874 he was offered the professorship of physics at the laboratory at Jena, but this he declined because he had already taken a position in the firm of Zeiss. He was made a partner in the firm and for the rest of his life devoted himself entirely to its interests, being, after the death of Carl Zeiss in 1888 and the retirement of his son Roderick Zeiss in the next year, the sole proprietor of the optical works, which he since converted into a co-operative trust. Many and great improvements in microscopes are due to him, and the construction of "Jena" glass by the firm of Schott & Co. is mainly a result of his action.

A SIXTH SATELLITE OF JUPITER.—The first Kiel telegram of the year brought remarkable news. Prof. Perrine, of the Lick Observatory, has discovered that Jupiter has a sixth satellite, much farther distant from the planet than any of those already known.

The first message said that the satellite was suspected in December, and that the suspicion was confirmed by an observation on January 4, when the position-angle of the new object was 269°, distance 45′, decreasing 45″ daily, and that the apparent motion was retrograde. A second message added that the discovery was made with the Crossley reflector, that the satellite was estimated to be of the 14th magnitude, and that observations were made on December 3, 8, 9, and 10, 1904, and January 2, 3, and 4, 1905. The distance, 45′, corresponds at the distance of Jupiter to about six million miles, and since the given rate of decrease implies that the angular distance at greatest elongation would be considerably more than 45′, and as inclination of the apparent orbit, about which no information at present is to hand, might make the apparent distance smaller than the actual, six million miles may be considered as a minimum estimate. Taking this as the mean distance, the resulting period is about 200 days, whilst the corresponding period for Satellite IV. is about 16¾ days, and distance 1,167,000 miles.

It may not be inappropriate to call attention to a paragraph in the Oxford Note-Book of last December, where the writer says, in speaking of the new satellite of Saturn, "It certainly seems as if an outer satellite of Jupiter should be looked for." And also to a note in last month's number about the Crossley Reflector.

FURTHER NOTE ON THE SIXTH SATELLITE.—When the hypothesis * had nearly been adopted that this object was only an asteroid that happened to linger in Jupiter's neighbourhood, another message was received from the Lick Observatory, which seems to restore its character. Why, however, were we kept so long in suspense? The observation was made on January 17, and not telegraphed till January 26.

The *Astr. Journ.* gives the distance on January 4 as 49′, not 45′, as the first announcement gave it. Assuming that the 49′ is correct, and taking the place on January 17 as distance 36′, position-angle 265°, a circular orbit has been deduced by Mr. Crommelin, which gives the distance from Jupiter as 7,000,000 miles, and the period as 248 days. West elongation took place on 1904 December 17, when the apparent distance was 58′·6. Conjunction will take place in the middle of February, when the satellite will probably be some 9′ south of Jupiter. A remarkable feature is the large inclination of the orbit to Jupiter's equator, which seems to be about 30°. This would appear to indicate that the object is a capture rather than an original member of the Jovian system. The phrase "apparent motion retrograde" in the original telegram should probably be taken as referring to the diminishing position-angle, not to the direction of orbital motion.

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* See Minor Planet Notes.

SATELLITES OF PLANETS.—The following letter from Prof. Oliver Lodge appeared in *Nature* of January 26:—

> May I ask whether the small distant eccentric and possibly retrograde satellites of Jupiter and Saturn which have been discovered, and seem likely to be discovered, ought not more properly to be regarded as cometary bodies or a shoal of meteors not yet too much drawn out for visibility at a distance? Would it not be possible for the larger planets to be attended by such bodies, the orbits of which have been made moderately elliptical by an accidental perturbation? It is known that the larger planets are able to capture comets for the Sun; is it possible that with the aid of their satellites and subsequent tidal action that they may be able to catch a few for themselves?

A LARGE SUN-SPOT.—On the photographs of the Sun taken at Greenwich on Sunday, January 30, there was seen an immense spot near the eastern limb, so that it appeared to have come on to the visible disc on the day previously, when no photograph had been taken, as the day was overcast. The heliographic co-ordinates are approximately longitude 310°, latitude −16°, and the spot, which is easily visible without a telescope, extends over six degrees of solar latitude and thirteen of longitude. Allowing for foreshortening, the spot, which on January 30 appeared as a compact ellipse, covered at least two thousand-millionths of the Sun's visible hemisphere.

METEOROLOGY IN JANUARY.—The weather of the past month has been remarkable for its sudden changes. The year began with a day colder than any of the preceding twelve months, but the cold snap lasted only until the second day, and January 4 was remarkable for its equable temperature, for the thermometer-reading at Greenwich ranged only from 48° to 45°. The thermometer record on Jan. 16 was remarkable. At 8 A.M. the mercury stood below 21°, at noon it was about 23°, but at 9 o'clock in the evening the wind, which had been E.S.E., changed to S.S.W. and the thermometer rose to 43°·5 by midnight. The last week of the month has been remarkable for the barometer-reading, which was above 30 inches from the morning of Jan. 21 to Jan. 31, when it began to fall. The highest reading 30·78 (reduced to 32° F., but not to sea-level), which has been exceeded only once since regular meteorological observations began at Greenwich (the exception being in 1882 January), was reached at midnight on January 28.

THE NUMBER OF THE NEBULÆ.—One scheme of work designed for the Crossley Reflector by the late Prof. Keeler was the taking of a series of photographs of 104 of the brighter nebulæ and clusters in all parts of the sky. This work has now been done, and has led to the detection on the plates of 745 new nebulæ in 57 of the regions, all of them very small and faint. Making an estimate, the results indicate 500,000 as the corresponding number of nebulæ within reach of the Crossley Reflector.

January Fireballs.—Mr. Denning, writing to us with regard to January 9–13 being a prolific fireball epoch (see *Observatory*, 1905 January, p. 52), says that the following observations have reached him:—

1905 January 9, $10^h\ 9^m$.—Mr. J. P. Kenyon, of Stockport, saw a brilliant meteor, about equal to Jupiter, moving slowly from $335^\circ + 80^\circ$ to $22^\circ + 45^\circ$ in 5 or 6 seconds; colour bluish yellow; no train. [Like the large meteor of 1903 January 13, this object was directed from γ Boötis, but it is quite possible that they may have been late members of the Quadrantid shower.]

1905 January 14, $10^h\ 15^m$.—Mr. T. Callard, Jun., of Ammanford, Carmarthen, saw a splendid meteor passing rapidly from S. to N. between the Polar star and Pointers. Duration 4 secs.; head electric blue, centre white, and tail very red.

1905 January 14, about 10^h P.M.—Dr. C. P. Hooker, of Cirencester, observed a meteor of great brilliance travelling across the sky in a N.W. direction. The train was of vivid white light.

The last two descriptions evidently refer to the same object, but they are not sufficiently definite as to the observed path to be really valuable.

The Solar Parallax.—A small preliminary determination of the solar parallax by the diurnal method, from six photographs of Eros taken with the Crossley Reflector at the opposition in 1900, is published in 'Lick Observatory Bulletin' No. 64, which gives $8''\cdot788$ as the resulting value of the parallax, with a probable error of $\pm 0''\cdot008$.

The Eros Photographs.—We regret that a note under this heading on page 71 of our last number is open to some misapprehension. The table of differences between the positions of stars as found on the Paris and the Algiers plates was not discovered by Prof. Turner, but is given by M. Trépied in the Circular. It should, besides, have been pointed out that Mr. Hinks, in his paper in the 'Monthly Notices' of June last, had already pointed out an error of this order and kind in the Algiers plates, and had assigned a cause for it which is not "magnitude equation" in the sense that the term is generally used.

On the first page of the *Companion* for 1905 mention was made of the increase in the number of Variable Stars. In the *Companion* for 1895 the catalogue contained 224 stars, and the number of Algol-type variables was 10. This year the corresponding numbers are 480 and 34. The increase is not surprising when one notes the wholesale way in which variables are being discovered. Harvard Circular No. 90, dated December 8, is headed "105 New Variable Stars in Scorpius"; Circular No. 91, of same date, gives details of 16 new variable stars in Sagittarius, several being of the Algol type. Altogether, from examination of Harvard photographic plates, Miss Leavitt has discovered 410 variables.

'SYMONS'S METEOROLOGICAL MAGAZINE,' in which one can usually find something amusing as well as a good deal of interesting meteorological information, is to appear in a cover of new design, illustrated by a picture of the Tower of the Winds at Athens, which it is thought will appear more attractive to chance readers than the one hitherto used. The Editor, Dr. H. R. Mill, is hopeful of a larger circulation, necessary for the enlargement of the magazine, and in this matter he has our sympathy and best wishes.

AT the St. Louis Exhibition a bronze medal was awarded to Hobart College, Geneva, New York, for its general education exhibit, and a special gold medal for its astronomical exhibit, which was prepared by Prof. Brooks, and consisted of a collection of photographs of all the comets, twenty-four in number, that he has discovered.

WE understand that Mr. Walter Maunder, who has conducted the astronomical department of *Knowledge* since the death of Mr. A. C. Ranyard in 1894, has resigned his connection with that Journal.

PHŒBE, the ninth satellite of Saturn, was shown on a series of photographs taken with the Crossley Reflector last November. The satellite is said to have appeared of about the 17th magnitude.

THE next Meeting of the Royal Astronomical Society (the Annual General) will take place on Friday, February 10, at 5 o'clock. The next Meeting of the British Astronomical Association will be on Wednesday, February 22.

FROM AN OXFORD NOTE-BOOK.

Oxford, 1905, Jan. 20.

THE latter part of my journal did not get written at the proper time, owing to human weakness; and it becomes necessary to compile it from notes and recollections after an interval of several months, during which much has happened. For instance, a sixth satellite of Jupiter has been discovered and (if we understand the telegram rightly) may be going round the unexpected way, as in the case of Phœbe. My hearty and admiring congratulations to Mr. Perrine. He was very reluctant to come with us to St. Louis, because the new mounting for the Crossley reflector had just been completed and he thirsted to be at work: but "duty first and pleasure afterwards"; and so he came and took minutes of the Solar Conference with great self-denial, and with commendable accuracy considering that his heart was with a new telescope some thousands of miles away. But the Conference once over, he has lost no time in showing us what the thrice-renovated Crossley reflector can do: and we shall expect to hear still more from him before long.

THE most remarkable fact about the St. Louis week was that we had fires on two evenings. After all that we had heard about our being broiled alive, we found a cool English climate and a desire for artificial heat. We could not have had a more misleading meteorological forecast, even for a total eclipse. The disappointment was, however, met heroically, and no doubt if mosquitoes had failed us also, the same fortitude would have been exhibited; but the majority of those who attended the St. Louis Congress were not called upon to bear the double disillusionment. Major Ross, who was there to read a paper on the distribution of malaria round a centre (fancy getting mathematics into the mosquito problem!), told me that efficient draining of all shallow ponds, such as is well within the resources of a great city, would exterminate mosquitoes in the neighbourhood. But the time is not yet.

THE "opening exercises" of the Congress of Arts and Sciences took place on Monday, September 19, at 3 P.M., in the magnificent reception hall of the Exposition, which is perhaps already being demolished. The temporary builings at St. Louis, which were explicitly to be removed in six months, cost the enormous sum of 27 million dollars, nearly twice as much as was paid to Napoleon for the vast territory acquired by the "Louisiana Purchase" a century ago. They were truly on a gigantic scale: in the Agricultural building alone it was possible to walk for nine miles among the exhibits without treading the same track twice. The hall in which we assembled contained the largest organ ever built, and we learnt with interest that it had been built in Los Angeles, and was to find its home in that terribly desolate-looking place—Kansas City. The 1000 people who met for the "opening exercises" filled the front benches, but not much more. There were 100 foreign delegates present.

FORMAL speeches from the Exposition officials explained the origin of the Congress, and an address of welcome to the foreign delegates was replied to by one representative from each country. Sir W. Ramsay responded for England. Noteworthy was the little speech of Prof. Brunialti, who, after a few hesitating words in English merely begging permission to use his own language, burst into a rhapsody of eloquent Italian. The *pièce de resistance* of the programme on Monday was, of course, the Address of the President of the Congress, Simon Newcomb. I extract the following from the *Times* of December 27:—

The main point of the address was to claim a pre-eminent place for the scientific investigator. "Intellect is the *primum mobile*, and not the material circumstances to which attention is so often directed." "While giving all due honour to the great inventors, let us remember that the first place is that of the great investigators whose forceful intellects opened the way to secrets previously hidden from men." In a rapid historical survey of the advance of knowledge, Professor Newcomb drew attention to the chief critical epochs when some great idea had opened a new floodgate; or, in terms of another analogy, when some ship, the patient building of which had been going on for years, was finally launched.

THE Congress of Arts and Sciences forthwith broke up into a large number of smaller Congresses. Perhaps the weak point of the whole idea was the failure to estimate beforehand the impossibility of securing any real existence for the Congress as a whole. We met as a body at this preliminary gathering, and at one or two most enjoyable dinners and receptions (though then not quite completely); but we were all so busy with our own Sections that there was little opportunity for hearing about other departments of knowledge. The same may be said of all meetings of this type; for instance, of the British Association. But we had nothing corresponding to the two public lectures which form a welcome feature of the B. A. gatherings. The problem of the re-unification of the sciences is no doubt extremely difficult, and the Congress achieved something of value by bringing its difficulties more prominently before us. And, after all, a good deal was accomplished by informal interviews and conversation which was perhaps impossible to secure by more definite arrangements.

THE Astronomy Section was addressed on Tuesday afternoon by E. C. Pickering and Lewis Boss. We met in a building with the significant name "Battle Abbey"; and Prof. Pickering had not advanced far in his account of Stellar Photometry when the battle began outside. He regarded his audience with a comic look of despair, and asked us to come closer round him: we clustered as close as we could, and so, to the accompaniment of a steady fusillade, we listened to the account of the wonderful work done at Harvard in measuring stellar brightness and discovering variable stars. It was in other respects a happy moment to give such an address: for the Director was able to announce the quite recent discovery of *hundreds* of new variables at Harvard. New mines of wealth had been found in the nebulous regions, and a beautiful way of working the mines by superposing two plates—a positive and a negative—had got out the ore in abundance. Some week or two later I had the pleasure of trying my "prentice" eye at the method, under the superintendence of Miss Leavitt, and was fortunate enough to earn her approval by re-discovering some of the more obvious variables she had found. [There was a momentary excitement when it seemed as though I had found a new one; but it turned out to be a fault in the plate.] The experience impressed me more forcibly than ever; if that is possible, with the value of the material accumulated at Harvard. I doubt whether there is an astronomer living who could not profitably spend the remainder of his life among those photographs alone.

But to return to the Astronomical addresses. Prof. Boss spoke of the positions of the stars, and of our rapidly maturing knowledge of their distances. Our annual orbit round the Sun, he said, is perhaps too small to give us a base-line; but the Sun, in his journey through space, is providing us with an ever-increasing base for stellar parallax: and before another century is completed we may hope to *see the stars standing out in three dimensions.* But we are at the moment full of hope that Prof. Boss may come to England next month to receive the Gold Medal awarded him by the Royal Astronomical Society: so I will say no more of what he will perhaps be able to tell us so much better himself.

Next day we heard Dr. Backlund, Prof. Kapteyn, and Prof. W. W. Campbell. I was unfortunate enough to miss the greater part of the first address by mistaking the place of meeting. I went back to Battle Abbey, only to find a small audience waiting for an address on Sanskrit. When I ultimately found my colleagues, Dr. Backlund had nearly finished; but I was in time for Prof. Kapteyn's interesting account of his researches on stellar parallax from their proper motions, which have led him to conclude that there are two streams of stars meeting each other in space. Prof. Campbell, in the afternoon, gave a masterly *résumé* of the present state of spectroscopic knowledge.

Besides these main addresses, there were a number of "ten minute" papers which were none the less important because of their brevity. We heard that the U.S. Naval Observatory is establishing a new observing-station at Tutuela, in Samoa; of the Survey for Double Stars being conducted at the Lick Observatory; of the completion of the Lunar Theory on Hill's plan by Prof. E. W. Brown; of eclipse problems; and of a number of other matters of which I fear I possess no adequate reminders. I gather that the printing of all the papers is under way, and it will then be possible to read the addresses more at leisure. There was no discussion on any of the papers, owing to lack of time: we had the tantalized feeling of railway-travellers who see a noble banquet spread, but are only allowed five minutes to eat it. Nevertheless, five minutes may be used to good advantage by determined people who know there are no more; and we worked at fairly high pressure.

The Astronomers did particularly well, for they got into the week a notable addition to the programme in the shape of a

Conference on Solar Research. This was only rendered possible by the great energy of Prof. Hale; but he had made such excellent preliminary arrangements that a good deal was accomplished in a couple of hours. The minutes of the proceedings will be printed by the U.S. National Academy of Sciences, and will also appear in the *Astrophysical Journal*; but it may be briefly stated that it was determined to form an International Committee for organizing solar research, composed of one representative from each of the Academies, Societies, or Committees which had given in their adhesion to the scheme; that the approval of the International Association of Academies was to be asked; and that a meeting of the Committee should be held in the summer or autumn of 1905 to formulate a definite plan of operations. Meanwhile Prof. Hale, as the Chairman, represents the executive.

On Friday there was a section on "Cosmical Physics," and I will again extract a paragraph from *The Times* of December 27:—

> "Cosmical Physics" is a department of science which is rapidly growing in favour. As a subsection of the British Association, for instance, it is scarcely three years old, owing its birth to the fusion of the existing subsection of astronomy with non-existent sections for meteorology, seismology, and magnetism. But once born it has justified its existence by growing rapidly and healthily, and has apparently fascinated the readers of papers and addresses in the main section. Dr. Arrhenius devoted his address to emphasizing the intimate relationships of all the sciences above mentioned among themselves and with others. For instance, meteorology was closely concerned with the amount of carbonic acid gas in the atmosphere, which now remained nearly constant; but had the quantity always been constant? If it had formerly been much greater, how had plants lived? There was some evidence—not very much, unfortunately, but still some—that certain forms of plant life could exist in an atmosphere strongly charged with carbonic acid gas, perhaps in an atmosphere of that gas alone. More light was needed on this question, so that physiological chemistry must come to the aid of meteorology. Not the least interesting feature of the address was the genial personality of the great Swedish philosopher himself. Spectacles in one hand and a few notes in the other, to which he only occasionally referred, he spoke fluently in a language not his own for nearly an hour, seeming to be totally unmoved by interruptions arising from the constant accession of new auditors. Before the end of his address the room, which was well filled at the beginning, was closely packed with an eager audience.

No one who has had experience of American hospitality will doubt that everything possible was done to make our visit pleasant. The citizens of St. Louis must have had years of worry and expense in getting up the Exposition (I believe their own subscriptions as private individuals came to five millions), and a wearisome succession of guests of all kinds for many months; and yet they were as cheery and hospitable as though they had lived in a desert and we were the first friends they had seen for an age. The American Astronomers insisted upon entertaining us at dinner when the citizens would let them, and when there was no public banquet, as there was on two nights during the week. At the second of these banquets, by the way, there was a rather amusing incident, the account of which I will again take from *The Times*:—

> The second dinner was made noteworthy by an unrehearsed effect. It was held in the dining-hall of the Tyrolean Alps, and the national feeling of the Garde Républicaine band, which had been engaged to play at the Exposition when required, was unfortunately aroused. In the first instance they refused to play at all, and it was only at the express command of the French Commissioner (who rose from a sick-bed to interview them) that they entered the hall. Then their opportunity came. The toast of "All Nations" was drunk and national airs were called for. No one who heard that band play the "Marseillaise" is likely to forget it; for the acoustic properties of the hall are good, and excited performers on brass instruments were able to use them to great advantage. The English National Anthem followed, cordially enough; but when "Die Wacht am Rhein" was called for no copies of the music could be found, which is perhaps excusable. But it seems clear that a little care must be exercised if these international congresses are to be really, as Mr. Bryce declared, "a great factor in the interests of peace."

After the week at St. Louis the foreign delegates of the Congress visited Washington, Boston, and New York. At Washington we were cordially received by President Roosevelt, who first shook hands with us all, and then spoke a few carefully chosen words to the effect that no nation at the present time could afford to be isolated, but must play its part in the world's affairs; and he regarded our presence as a practical recognition of this fact. Thus, without mentioning politics, he illuminated his speech with light from the lamp of Imperial Policy in a skilful manner. In the two evenings spent at Washington we were delightfully entertained—first, at the Cosmos Club; and secondly, by Professor and Mrs. Newcomb and the citizens of Washington.

At Harvard we had other receptions of the pleasantest kind; and the opportunity of seeing American football played, which reminded us how autumn had gradually come upon us during our protracted rambles across the Continent. We saw the pit dug in the Observatory grounds for the reception of the floating-polar-axis of Common's Five-foot, which was reported as on its way to Harvard, and learnt that Prof. Pickering proposes to adopt a coudé form for it, so that the observer looks permanently down the polar axis. Possibly it is now nearly ready for work.

But the time has come to conclude these notes of travel. How many things well worth recording have escaped record I feel only too keenly. Those who have crossed the Atlantic know full well how much there is to see and learn, and how many pleasant things to do. Those who have not yet done so we can, without a misgiving, urge strongly to take the first chance that may come in their way of repairing the omission. It is true that they may find a whole pile of arrears awaiting their return; but this is a mere detail compared with the benefit and pleasure to be derived from the expedition.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1905 February 10.

Prof. H. H. TURNER, D.Sc., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

Mr. Whittaker read the Minutes of the last Annual General Meeting, which were confirmed.

Mr. C. Thwaites presented the Report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1904, and found and certified the same to be correct. The cash balance in hand on 1904 December 31 was £262 5*s.* 2*d.*, as compared with £260 16*s.* 3*d.* on 1904 January 1. They had inspected the books, instruments, and other effects in the possession of the Society, which appeared to them to be in good condition.

The Secretaries then presented the Report of the Council to the Society.

Mr. Whittaker read extracts from the Reports of Observatories, which included, besides the Royal and University Observatories, those belonging to the following amateur astronomers:—(The late) Mr. E. Crossley, Rev. T. E. Espin, Mr. Saunder, Mr. A. Roberts (South Africa), the late Dr. Roberts (*Observer*, Mr. Franks), and the Rousdon Observatory (*Observer*, Mr. Grover).

Mr. Dyson read extracts from the obituary notices of Fellows who had died during the past year. The list contained the names of the following Fellows:—William Anderson, John Mackenzie Bacon, Reginald Bushell, Robert Prichard Davies, William Francis, J. Horsley Haslam, Rev. Andrew Henderson, George E. Lumsden, Frank McClean, William Grant MacGregor, William Noble, Sir

Erasmus Ommanney, William Montgomery Pierson, Eyre Burton Powell, Walter J. B. Richards, Isaac Roberts, Maurice Allen Smelt, John Steele, Sir Henry Thompson, and one Associate, Theodor Bredichin. Obituary notices were also given of the following, who died in January 1905:—Edward Crossley, Charles Horsley, Paul Henry (Associate).

Mr. Whittaker read the list of the subjects reported on in the Progress of Astronomy during 1904:—Meteoric Astronomy, The Comets of 1904, A Comparison of the Features of the Earth and the Moon, Saturn's Ninth Satellite Phœbe, Discovery of Minor Planets in 1904, Double Stars, International Co-operation in Solar Research, New Observatory at Mount Wilson, Universal Time and Longitudes, Variable Stars, The Astrographic Chart, and Solar Activity in 1904.

The President announced that the Gold Medal of the Society had been awarded to Prof. Lewis Boss, Director of the Dudley Observatory, Albany, New York, for his long-continued work on the positions and proper motions of fundamental stars, and delivered an Address setting forth the grounds on which the award was made. Whilst the Address was being delivered, the Hon. J. H. Choate, the American Ambassador, entered the room and took a seat on the front bench. Prof. Turner concluded his Address with these words:—

"At the beginning of this Address I referred to our list of Medallists as in some sort an epitome of astronomical history during the past 85 years. One of the features of that history, which gives us as Englishmen peculiar satisfaction and which is faithfully reflected in this list, is the steady and rapid development of astronomical work in the United States. At the foundation of our Society it could scarcely be said to exist; at the present day its growth is so vigorous that for the third time within five years we have the privilege and pleasure of recognizing the merits of an American astronomer. . . . The journey across the Atlantic at the present time of year is a serious one, and we hear without surprise, though with rather selfish regrets, that our Medallists cannot interrupt their work to undertake it. In their absence it has been a great pleasure to us to be honoured by the genial presence of Mr. Choate, and it is particularly kind of him to-day to spare us a few moments from the busy weeks preceding a departure which the whole of England unites in deploring."

Then addressing the Ambassador:—

"Mr. Choate, in sending this Medal to Prof. Lewis Boss, will Your Excellency kindly assure him of our great admiration of his long-continued work and our deep sense of its fundamental importance to astronomy, and our earnest hope that means may be found for carrying into effect his splendid plans for the future?"

The Hon. J. H. Choate. It seems but yesterday that I attended in this room to receive this same Medal for Prof. Pickering, of the Harvard Observatory, and this is the third time that I have been here for a similar purpose. It shows that our astronomers at home are too devoted to their profession to bask in this scientific light. It is so often that I have been called upon to do a similar service at this or the Geographical and other Societies, that the receiving of medals seems to have been a prominent feature in my career as Ambassador here, now drawing to a close. Your President kindly sent me an advance-proof of his eloquent Address, which I read carefully, for I must confess it required some care to bring its meaning home to my unscientific mind, but it left the impression that Prof. Boss, with extreme accuracy and undying energy, has been pursuing his profession in a way that has earned for him the honour that you have this day bestowed upon him. Whilst this learned Society has acknowledged the merits so lovingly set forth by your President, I can assure you that his fellow-citizens are no less proud of him. I received a letter a day or two ago from Mrs. Pruyn, of Albany, expressing her appreciation and that of many other inhabitants of that city of the honour bestowed on the Director of the Dudley Observatory, an element in whose character is his conspicuous modesty, and all enlightened Americans understand that this Medal is the crowning reward that an astronomer can receive for his work.

The President. The Jackson-Gwilt Gift and Medal have been awarded to Mr. John Tebbutt, of Windsor, New South Wales, for his important observations of comets and double stars and his long-continued services to Astronomy in Australia, extending over forty years.

This Gift has only been awarded twice previously—in 1902 to Dr. Thomas Anderson, and in 1897 to Mr. Lewis Swift. In announcing the award in 1897 the President stated that after some consideration he had decided not to give an address in connection with the award, and I shall follow the precedent thus set. But there can be no reason why I should not recall to your memory that Mr. Tebbutt began astronomical work in 1854, and is now only relinquishing it at the age of 70; that, amid surroundings which gave him little encouragement, he has made regular and systematic observations during half a century, including, for instance, those of some 1400 occultations of stars by the Moon, and valuable measures of double stars; that he has contributed over 80 papers to the 'Monthly Notices'; and has, moreover, discovered several comets, including notable ones of 1861 and of 1881, the orbits of which he computed from his own observations.

In handing this Medal to the Secretary for transmission with the Gift to Mr. Tebbutt, I will ask him to convey our hearty congratulations to the recipient on the accomplishment of half a century of single-handed astronomical work for which it would be difficult, if not impossible, to find a parallel, and our delight that Mr. Tebbutt should be entering upon the rare enjoyment of a thoroughly well-earned period of rest.

Mr. Choate then left the Meeting.

It was proposed by *Lord Rosse*, seconded by *Mr. Inwards*, and carried unanimously:—"That the Report of the Council be received and adopted, and that it be printed and circulated in the usual manner, together with the Report of the Auditors and the President's Address."

The President. It will soon be my pleasant duty to vacate this Chair to make room for the new President. Before doing so, may I presume the existence of a few minutes' interregnum which will lend themselves to a semi-official act. I recently brought to your notice by circular letter the fact that our valued Assistant-Secretary, Mr. Wesley, was on the point of completing 30 years' service, and I ventured to suggest that we should recognize this occasion by a small presentation to him. I am glad to say that my suggestion met with a most hearty response. The letter was issued only to the 500 Fellows living in the United Kingdom, and of these 160 have already replied. The stream of replies has been running freely, and I hope I may without impropriety allow it to continue for the benefit of those who, by temporary forgetfulness, might be deprived of the privilege of joining in this recognition of Mr. Wesley's services *.

I have asked Mrs. Wesley to be present on this occasion, not only for obvious reasons, but because I have a particular acknowledgment to make. I feel that when an idea is taken up and proves successful, the originator of the idea should not be forgotten, and, remembering this, I ought to say that Mr. Knobel was actually the originator of this appreciation of the work of Mr. and Mrs. Wesley. But I was going to say that we have lately ventured on what bids fair to be a successful experiment, viz. the occasional Evening Meetings of the Society, and I should like the Fellows to know that the idea of such gatherings is due to Mrs. Wesley; and when we consider the extra and special trouble they entail on her, I think the fact is significant as showing how keenly alive she is to all that concerns our welfare and comfort.

Mr. Wesley, in the name of the Royal Astronomical Society, I thank you for your devotion to its interests during thirty years: and, on behalf of a large section of the Fellows, I hand you this purse as a small token of our appreciation of the manner in which you have unsparingly and unfailingly satisfied our miscellaneous wants; of our friendship and regard for you; and of our earnest hope that it may be long indeed ere we are under the necessity of looking for—what will be so difficult to find—a successor to yourself.

Mr. Wesley. I have hardly words to thank you for your kindness, which is as undeserved as this expression of it was unexpected. In the thirty years—nearly half my lifetime—that I have held this office, I have simply tried to do my duty, and no man deserves to be rewarded for that. But though I am not able to speak of myself, I must thank you, Sir, for the kind way you have spoken of my wife. I know that Mrs. Wesley has never failed to do her utmost for the comfort of the Fellows of this Society. I can only say once more how much I thank the Fellows of the Society for this testimony of their esteem, which, if unmerited, is none the less delightful, since it makes me believe that you are satisfied with what I have done.

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* [The list is still open for the receipt of further contributions.—Eds.]

The President appointed Mr. Thwaites, Mr. Seabroke, and Capt. B. F. Stevens as Scrutineers of the Ballot for the election of Officers and Council for the ensuing year. The Ballot was then taken, and the following were declared duly elected :—

President: W. H. Maw. *Vice-Presidents*: Sir W. H. M. Christie (Astronomer Royal), Major P. A. MacMahon, H. F. Newall, Prof. H. H. Turner. *Treasurer*: Major E. H. Hills, C.M.G. *Secretaries*: Thomas Lewis, E. T. Whittaker. *Foreign Secretary*: Sir William Huggins, K.C.B. *Council*: Sir William Abney, Sir R. S. Ball, Rev. A. L. Cortie, P. H. Cowell, F. W. Dyson, Alfred Fowler, J. W. L. Glaisher, A. R. Hinks, E. B. Knobel, A. A. Rambaut, S. A. Saunder, E. J. Spitta.

Mr. W. H. Maw then took the Chair.

Mr. Cottam proposed and *Mr. Seabroke* seconded :—"That the thanks of the Meeting be given to the President, Vice-Presidents, and Secretary who now retire for their services in promoting the objects of the Society."

Mr. Maw, in putting the motion to the Meeting, said :—It is very gratifying to me that my first task as occupant of this Chair should be such a pleasant one. I should like to say, and to emphasize the remark, how well we have been served by the retiring President during the last two Sessions. I think I am expressing the sense of the Meeting by saying that this is no mere formality, but that we heartily thank the late Officers for their services.

The motion was carried unanimously.

A vote of thanks, proposed by *Mr. Newbegin* and seconded by *Mr. D. G. Simpson*, was accorded to the Auditors of the Treasurer's Accounts and to the Scrutineers of the Ballot.

The following gentleman was elected a Fellow of the Society :—

William Edward Raymond, Sydney Observatory.

The following Candidates were proposed for election as Fellows of the Society :—

William Bowyer, Royal Observatory, Greenwich (proposed by T. Lewis).

Rev. T. J. Charlton, The Rectory, Omeath, Co. Louth, Ireland (proposed by Dr. J. S. Slater).

Capt. L. A. D'Emres, Marine and Fisheries Department, Ottawa, Canada (proposed by Capt. P. Thompson).

D. J. R. Edney, Royal Observatory, Greenwich (proposed b W. W. Bryant).

Herbert Henry Furner, Royal Observatory, Greenwich (proposed by F. W. Dyson).

J. A. Parkhurst, M.Sc., Yerkes Observatory, Williams Bay, Wis., U.S.A. (proposed by H. H. Turner).

Montagu Austin Phillips, 22 Petherton Road, Highbury New Park, N. (proposed by F. W. Levander).

A. L. Wood, H.M.S. 'Conway' (formerly of the Royal Observatory) (proposed by W. G. Thackeray).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 January 25.

President: A. C. D. Crommelin, B.A., in the Chair.

Secretaries: J. A. Hardcastle.
J. G. Petrie.

The Minutes of the previous Meeting were read and confirmed, and the list of presents received was read, and the thanks of the Meeting were accorded to the respective donors.

The names of two Candidates were read and passed for suspension, and the election by the Council of six new Members was unanimously confirmed.

Mr. W. T. Lynn read a paper on "The Eclipse of Agathocles." The writer referred to a paper by Prof. Newcomb, read at the last Meeting of the Royal Astronomical Society, in which, on the authority of Prof. Celoria, this eclipse was identified with one mentioned by Cleomedes. Mr. Lynn thought this identification could not be accepted. The statement of Cleomedes was very indefinite, but he would be more likely to be referring to an eclipse of his own time than to one three centuries before it, and, further, he speaks of the amount of obscuration as having been measured at Alexandria. Now that city had certainly been founded before B.C. 310, but it was not under the settled government of the Lagidæ for some time afterwards. The establishment of a seat of learning there began, in fact, with Ptolemy Philadelphus, who became sole king in B.C. 283.

The President said it was well known what an amount of research Mr. Lynn had devoted to these ancient eclipses, and he certainly had succeeded in throwing so much doubt on the identification of this eclipse that it would be quite unsafe to use it, as Prof. Newcomb had done, to correct the secular acceleration of the Moon. Prof. Newcomb, in his paper, stated that this eclipse compelled them to diminish the accepted value of the acceleration from somewhere between 8 and 9 seconds, at which he had taken it before, to about 6½ seconds; and, as he said, if they took the smaller value, that allowed nothing for the slowing down of the Earth's rotation through the tidal friction which undoubtedly was going on.

Mr. G. F. Chambers then read a paper on "The Spanish Eclipse." He said that, in accordance with an announcement which he made at a recent Meeting of the Association, he had taken steps to investigate the climatic conditions of certain Spanish towns which might be taken as furnishing a clue to the weather of localities likely to be visited by eclipse observers. The materials for the investigation had been kindly supplied by Señor Iniguez, of the Madrid Observatory. The conclusions which he had drawn from the figures were :—

(1) That the north coast of Spain was not quite so hopelessly unpromising as previous information had led him to expect.

(2) That the heat likely to be experienced in the south of Spain was not, after all, so very formidable.

(3) That if anybody was inclined to go to the Balearic Islands, and "to rough it," the sky prospects there would be very good.

Mr. E. W. Maunder did not think there was much to add to what had already been said with regard to preparations for observing the eclipse. The result of the Circular sent out since the last Meeting had been conclusive that it would be beyond their power to charter a special steamer. He hoped by next time to be able to submit one or two more modest schemes of routes to the central line. He thought that one expedition might be arranged to the north coast of Spain. Then the ordinary modes of communication with the island of Majorca were sufficiently frequent, and by no means expensive, and this was equally the case with regard to Phillipeville in Algiers.

Mr. Hardcastle read a paper by *Mr. C. T. Whitmell* upon "Eclipses of the Midday Moon." The paper might be regarded as complementary to one on "Eclipses of the Midnight Sun," published in the December 1904 number of the 'Journal' (vol. xv. pp. 86–91). On 23rd June, 1899, just after the northern summer solstice, occurred a total eclipse of the Moon, the Greenwich Mean Time of mid-eclipse being $2^h\ 18^m$ P.M. The declinations of the Sun and Moon were then about equal in magnitude but opposite in sign. Mid-eclipse would be visible at local midnight along an arc of meridian (146° E. of Greenwich) extending from latitude 66½° N. to the South Pole, and at local noon along an arc of meridian (34° W. of Greenwich) extending from the South Pole to latitude 66½° S., this second arc being a continuation of the first arc. This second arc therefore fulfilled the conditions necessary for observing an eclipse of the midday Moon. If they supposed that an eclipse, similar to the one already dealt with, had occurred at the northern winter solstice, then an eclipse of the midday Moon would have been visible from a certain arc of meridian extending from the North Pole to latitude 66½° N.; from this arc the Moon would be seen due north. An eclipse, nearly fulfilling the required conditions, occurred on 27th December, 1898, the Greenwich Mean Time of mid-eclipse being 11^h 42^m P.M.

The President remarked that Mr. Whitmell had shown great perseverance in trying to get out every possible form of eclipse. While these eclipses did not present the rare and topsy-turvy features of the midnight Sun eclipses, they were certainly rather a curiosity.

Mr. G. M. Seabroke then took the Chair while the *President* made a statement regarding "The So-called Sixth Satellite of Jupiter." He referred to the discovery by Prof. Perrine, of the Lick Observatory, of what he believed to be a sixth satellite of Jupiter at a distance of 45 minutes of arc, and with a position-angle of 269 degrees. It was stated to be approaching Jupiter at the rate of 45 seconds of arc daily, and its position-angle was diminishing. On the 24th of January, however, Prof. Wolf, at Heidelberg, had discovered by photography a minor planet near Jupiter, which was 75 minutes of arc E. of Jupiter and 45 minutes N., and it was suggested that this was the same object as was observed at Lick on January 4. He said that it would be a great disappointment if that should turn out to be the case, because the ninth satellite of Saturn was quite a new feature in the solar system, and a distant satellite of Jupiter would have confirmed or contradicted, as the case might be, Prof. Pickering's theory of the ancient retrograde rotation of the planets, according to which all very distant satellites ought to go round their primaries in a retrograde direction.

The President having resumed the Chair called upon Mr. Wigram to give some particulars respecting suitable Spanish stations from which to observe the forthcoming eclipse of the Sun.

Mr. Wigram exhibited an interesting series of lantern-slides illustrating a tour through Spain in or near the track of the coming eclipse. He said that Spain was a very attractive country to all who were not too fastidious as regarded their bodily comforts, but that, except in a few of the larger towns, the inns were still as in Don Quixote's day.

Mr. Maunder then showed a series of lantern-slides of stellar photographs taken, and presented to the Association, by Mr. Alexander Smith.

The President said that everyone present must have been struck with the extreme beauty of these photographs, viewed as pictures of the sky; in fact, as regarded mere beauty, they surpassed some of the pictures taken with much larger instruments.

Mr. Hardcastle read a letter from *Mr. Whitmell* upon "Halos and Rainbows," in which, in further reference to a paper by

Miss Stevens on this subject, he pointed out:—(1) That all true rainbows were concave to the direction of a line passing through the Sun and the eye. (2) That the only two rainbows bright enough to be seen, the primary and the secondary, were always situated in the quarter of the heavens opposite to the Sun, and that it was a decided mistake to call an arc, not due to rain, a rainbow, simply because the arc happened to be brightly coloured.

Mr. Maunder said it was an interesting fact that the ancient Babylonians had noticed the two classes of halos, and had measured their distances from the Sun, and had found them to be 22° and 46° respectively.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The usual monthly Meeting was held on Wednesday evening, February 15, at the Society's Rooms, 70 Victoria Street, Westminster, *Mr. Richard Bentley, F.S.A.*, President, being in the Chair.

Mr. E. Mawley presented his Report on the Phenological observations for the year 1904.

The weather of the Phenological year ending with November 1904 was chiefly remarkable for the persistent rains in January and February, the absence of keen frosts in May, the long continuance of hot and dry weather in July, and the small rainfall during the autumn. Throughout the year, wild plants came into flower behind their usual dates, but at no period were the departures from the average exceptional. Such spring migrants as the swallow, cuckoo, and nightingale made their appearance in this country at as nearly as possible their usual time. The yield of wheat per acre was the smallest since 1895, while those of barley, beans, and peas were also deficient. On the other hand, there were good crops of oats, potatoes, and mangels. The best farm-crops of the year were, however, those of hay, swedes, and turnips. Both corn and hay were harvested in excellent condition. Apples were everywhere abundant and all the small fruits yielded well, especially strawberries, but there was only a moderate supply of pears and plums.

The other papers read were: "Observations of Meteorological Elements made during a Balloon Ascent at Berlin on September 1, 1904," by *Dr. H. Elias* and *Mr. J. H. Field*; and "The Winds of East London, Cape Colony," by *Mr. J. R. Sutton.*

A Comparison of the Features of the Earth and the Moon *.

It has been truly said that the selenographer has rarely satisfied himself, and never satisfied his fellow workers, with the explanations he has suggested as to the origin of the features his map delineates. This perhaps is not to be wondered at when we remember what great want of agreement there has been amongst geologists as to the processes by which the surface of the Earth has been shaped; and yet the geologist has two distinct advantages over the selenographer: he can examine the Earth on the spot and can dig down into it, and he is a specialist working on the lines for which he has been specially trained. The selenographer, on the other hand, examines the Moon under considerable difficulties and from a great distance, he can only see its surface, and he is almost always an astronomer, frequently a self-trained amateur with a comparatively small telescope, and when he attempts to trace the course of events by which the surface has been brought to its present condition, he steps outside his particular domain and really invades the province of the geologist. Selenographers will therefore heartily welcome a work on this subject from a geologist as eminent as Prof. N. S. Shaler, of Harvard, who commenced his selenographical studies in 1867, nearly 40 years ago, and who has had the advantage of using an instrument as powerful as the 15-inch Harvard Merz refractor. Prof. Shaler has given a preliminary account of his conclusions in a number of the 'Smithsonian Contributions to Knowledge,' and this he hopes to supplement with a fuller treatise presenting the matters discussed in a more extended form. In order that this promised work may be as complete as possible he invites criticism, and it is in response to this request that the present article is written solely from the point of view of an amateur astronomer who takes great interest in the Moon, but can lay claim to no special knowledge of geology.

For those who have not read Prof. Shaler's book it may be well to commence with a brief outline of his principal conclusions †.

With regard to the different categories of walled plains, mountain rings, &c., into which selenographers have divided the most characteristic of the lunar formations, Prof. Shaler finds a continuous gradation from those more than 100 miles in diameter to the smallest visible in our largest telescopes, which may be some 500 feet across, and classes them all as "vulcanoids." He thus sweeps away a series of distinctions which have always seemed to the writer arbitrary and unsatisfactory. The eruptions by which these vulcanoids have been formed he believes to have been much less violent than those of terrestrial volcanoes; there were no explosions, but a quiet boiling or up-welling action of lava of a very viscid character. The first stage would be the raising of a dome-like elevation due to accumulation of vapours beneath the surface; at a certain period the top of the arch either fell in or broke outwards, in either case leaving a pit with lava inside. A kind of boiling in the interior would cause this lava to rise and fall, sometimes overflowing the rim, then quickly solidifying and so commencing the formation of a ring-shaped rampart. Fresh supplies of lava would melt down the interior face of the rampart and extend the diameter of the ring. Finally, as the supply of lava failed, that inside the rampart sank in several stages, the different levels at which it rested being marked by terraces so well shown in such craters as Theophilus and Copernicus. The last stage of activity would be marked by the formation of a small central crater, a low dome, or an extruded cone. The larger vulcanoids were the earlier, the size decreasing as the Moon cooled and the thickness of the crust increased.

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* By N. S. Shaler, 'Smithsonian Contributions to Knowledge,' No. 1438.

† This summary is quoted almost verbatim from an address given to the British Astronomical Association, 1904, Oct. 26.

Prof. Shaler considers and rejects the hypothesis, first suggested by M. Faye, that the rise and fall of the lava was due to the tidal action of the Earth, and also the hypothesis that the vulcanoids were formed by the impact of meteorites. He is, however, of opinion that the maria were formed by such impact of large bolides at a period subsequent to that at which the larger vulcanoids appeared, and possibly after the action which produced them had entirely ceased. The facts on which this theory is based are the dark colour of the floors of the maria, indicating a different origin from that on the floors of most of the larger vulcanoids; the uniformity of level, which indicates great fluidity in the lava, and is also strong evidence against its having flowed out from a central pipe or elongate fissure; the appearances at the borders of the maria, which seem to indicate that the lava first flowed over low areas and then retired, after the manner of waves produced in our own seas by great earthquakes; and, lastly, the absence of large vulcanoids from the floors, though there is evidence that previously existing formations have been overwhelmed and partially destroyed; this appears to indicate a late period of formation, and it is difficult to reconcile this with the sudden development of such large quantities of very fluid rock without some external source of heat, such as would be produced by the impact of a few bolides 5 or 10 miles in diameter. The small vulcanoids which are found on the surfaces of the maria he is inclined to attribute to boiling in the lava of the maria and not to the internal fluid of the sphere. Whilst the evidence is admittedly not complete, Prof. Shaler accepts this theory as the only working hypothesis he has been able to find which in any way serves to explain the existence of the maria. He believes that if any maria exist on the other side of the Moon they are less extensive than those on the part we see.

The elevations on the lunar surface, other than the vulcanoids, are divided into several different categories. First there are such ranges as the Alps, Apennines, and Caucasus, which consist of a number of elongate masses, connected at the bases, and which are held to be due to the expulsion of lava from the interior in much the same way as was suggested for the central cones of the vulcanoids. Then there are others, such as the Altai Mountains, with a steep escarpment on one side and a gentle slope on the other; these may have been produced by extensive faulting accompanied by the extrusion of lava, although in the clearest instance of true faulting—the Straight Wall—there was no such extrusion. These escarpments are perhaps the oldest features on the surface.

Then, again, there are the low ridges found on the floors of most of the maria, running frequently for 100 miles, and sometimes for two or three times as far, though they are not really continuous for more than 40 or 50 miles at a stretch; with these may be connected the wrinkles sometimes seen on the floors of the larger vulcanoids; these are of later date than the maria and indicate compressive actions of the type of those by which the terrestrial mountains were raised.

Valleys of the Alpine type are attributed to down-faults, or sinking of the surface caused by deficiency of support resulting from the extrusion of the material of which the mountains were formed. The rills, probably the latest of all the lunar formations, are due to shrinkage of the crust at a period when the cracks no longer extended down to a fluid interior.

The various ray systems and diffuse bright patches are attributed to the emission of some crystalline vapour through narrow crevices extending downwards to the interior of the sphere. This crystalline material is supposed to be of a phosphorescent nature, in order to account for the rays continuing to shine after the Sun has passed the angle at which they first become conspicuous.

Prof. Shaler does not admit the possibility of any present volcanic action on the Moon. All such he holds must have ceased long ago, probably before the Earth possessed even a solid crust. Nor will he allow the possible existence of even the lowest forms of organic life, such as have been adduced to explain the well-known periodic changes of colour on various parts of the surface.

Prof. Shaler makes no allusion to the theories which have been advanced by MM. Lœwy and Puiseux to account for the physical appearance of the Moon's surface, and indeed, from several indications, would seem not to be acquainted with them; but this makes it all the more interesting to note that his views as to the formation of what he calls the vulcanoids by a process of quiet upheaval rather than by a series of explosions are in general accordance with theirs. He does not, however, appear to have noticed the well-marked polygonal form which is assumed by so many of the larger formations—as, for instance, Ptolemæus, the directions of whose walls would seem to have an intimate connection with the valleys, ridges, and crater-rows to be seen on the exterior surface. This connection is well shown in Plate xxi. of Prof. Shaler's book, and is accounted for by MM. Lœwy and Puiseux by supposing that the Moon cooled in such a way as to establish lines of weakness and of strength in the crust. It would be interesting to learn Prof. Shaler's views upon this point.

With regard to the formation of the maria, the two systems exhibit a complete divergence of opinion, and it would seem to the writer that astronomers will require a very strong case before they admit the probability of a succession of impacts with bolides several miles in diameter. There is, as Prof. Shaler points out, no evidence of such bodies having ever impinged upon the Earth, and the only piece of astronomical evidence adduced, the recent discovery of Eros "at no great distance from the Earth," does not render such a series of catastrophes much more probable. It will be news to some that Eros is "apparently not to be classed either with planets or satellites." The view advocated by MM. Lœwy and Puiseux is that in consequence of the quantity of matter extruded in the formation of the vulcanoids, and in consequence also of shrinkage in the nucleus, hollow spaces had been developed, causing a loss of support over large areas of the surface, and that these consequently fell in, the fall causing the outflow of considerable quantities of lava. Many arguments may be found in Prof. Shaler's work that lend support to this view. Thus on p. 40 we read that "it appears likely that the north-west face of the Hæmus Mountains and the south-east face of the somewhat similar district lying between Eratosthenes and Mt. Hadley, facing the Mare Imbrium," are due to faulting on a large scale. By the latter region he clearly means the N.E. face of the Apennines. On p. 71 it is assumed that the lunar crust "abounds in cavities due to the ejection of the large amount of material contained in the ramparts of the vulcanoids," and it has already been noted that certain valleys are attributed to consequent faulting of the surface. And, once more, it would seem not unlikely that the falling in of such large areas would result in a flow of lava from greater depths than those from which that forming the vulcanoids was drawn, and this lava being at a higher temperature would be more fluid; for, as Prof. Shaler tells us on p. 40, the loss of a few degrees in temperature may cause a considerable rise in viscosity, whilst the great volume of the lava, together with the probable liberation of a considerable quantity of gas to form a temporary atmosphere, would both tend to retard the cooling, and to give time for the levelling of the surface on which Prof. Shaler so strongly insists. It would seem at all events desirable that this hypothesis should be thoroughly examined before resorting to the impact of bolides ten miles in diameter.

This meteoric theory has had a curious history. The earliest mention of it the writer has been able to find occurs in Proctor's book, 'The Moon,' published in 1873. Proctor does not explicitly claim it as original, but he makes no reference to any previous advocate, and he speaks of it as "the theory I have suggested"*, so that it seems quite likely this may be its first appearance. It is put forward with some diffidence; the author says, "I should certainly not care to maintain that as the true theory of their origin" (*loc. cit.* p. 345), and on the next page he expressly limits its application to the smaller craters. The theory was amended by Gilbert in his Presidential Address to the Philosophical Society of Washington in 1892 †, where, in order to account for the facts that the impacts must almost always have been normal, and at relative velocities less than the average velocities with which meteors from outer space would strike the surface, he supposed the Moon to have been formed by the aggregation of a ring of "moonlets" surrounding the Earth after the manner of Saturn's rings, the surface-markings being caused by the impacts of the last "moonlets" drawn into the main body. A similar theory has been quite recently advocated by Mr. G. Romanes in a paper read before the Royal Society of Edinburgh ‡. Gilbert's address is mentioned, but his special contribution to the theory is not alluded to by Prof. Shaler, who, in opposition to Proctor, rejects it for the smaller formations and accepts it only for the maria, and, in opposition to Gilbert, supposes the impacts to have taken place at undetermined angles and with very high velocities.

It is difficult to accept Prof. Shaler's several times repeated statement that none of the vulcanoids on the maria exceed 10 or 15 miles in diameter, or at all events none formed since the floors came to their present level. This cannot apply to such formations as Copernicus, Bullialdus, or Aristillus, which would seem to the writer to be on regions usually classed as maria, and, judging from their high state of preservation, to be among the more recent of the formations.

Again, on p. 59, in speaking of the bright ray systems it is stated that none of the distinct centres are found on the maria, and yet these centres must include Aristarchus and Kepler as well as Copernicus. The first-named is expressly stated on p. 53 to be on a mare, and on p. 58 to be a ray centre.

Prof. Shaler will not admit of the existence of any atmosphere upon the Moon; but he is certainly wrong when he says, on p. 69, that there is "no other valid reason for supposing that any kind of gas or vapour exists about the Moon save that it is deemed necessary to have it in order to explain certain changes of colour which are deemed to be evidences of organic life." In answer to this, it seems scarcely necessary to do more than quote the words of Neison, that "all astronomers who have devoted much time and attention to the detailed examination of the lunar surface have recognised more or less direct indications of the existence of a rare lunar atmosphere" *. But amongst these indications may be noted the appearance of twilight at the cusps seen by Schröter, Gruithuisen, Webb, the MM. Henry, and W. H. Pickering; an appearance of mist seen in and about various craters by Barnard, Klein, W. H. Pickering, and many others; a dusky band observed on Jupiter just before occultation at the bright limb by Barnard and W. H. Pickering, and actually photographed at Arequipa on 1892, Aug. 12; and a sensible flattening of the disc of Jupiter measured on photographs taken at Arequipa on the same occasion.

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* *Loc. cit.* p. 349.

† Bull. Phil. Soc. Washington, vol. xii. pp. 241–292.

‡ *Nature*, vol. lxxi. p. 143.

Prof. Shaler is not very well acquainted with selenographic literature, and that, indeed, is not to be wondered at, for such an acquaintance can only be acquired by laborious research, and it is not to be expected that one engaged in an altogether different branch of science should have hunted through all the original memoirs, of which there must be many whose existence is unknown to the present writer; but the absence of such knowledge might have caused some hesitation in making the criticism implied in the sentence quoted above.

Another instance of the same sort may be noted on p. 70, where, speaking of the controversy with regard to Linné, it is stated that variations due to difference of illumination, "combined, perhaps, with some original bad mapping and servile copying, may account for the seeming change." Prof. Shaler accepts the reality of a change as more probable than not; but it is hard to apply the charge of "servile copying" to any but Lohrmann, Mädler, or Schmidt, and the writer cannot but think that had Prof. Shaler himself looked into the history of the controversy and the work of these three most painstaking selenographers, the sentence quoted would never have been penned. So far as Schmidt is concerned, the following quotation would seem conclusive. He says: "On a general chart of the Moon, diameter 12 in., which I constructed from my own observations, probably about the end of 1840, I find Linné marked as a crater. The works of Lohrmann and Mädler were not accessible to me until the year 1843 at Hamburg" †.

Prof. Shaler comments on the small amount of evidence to be found on the lunar surface of compressive strains and the frequent evidence of tension, but makes no allusion to the explanation suggested by MM. Lœwy and Puiseux ('Comptes Rendus,' 1902, July 15) founded on a memoir by Dr. Charles Davison, in which it was shown that, when a globe is cooling steadily under uniform conditions, there is an outer shell subjected to pressure, but below this the material is in a state of tension. If, now, the radiation from the surface be increased, the layer from which the maximum loss of heat takes place—and which is therefore under tension—

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* 'The Moon,' p. 22.

† B. A. Report, 1867, p. 5.

rises and may reach the surface. The suggestion is that the disappearance of the much denser atmosphere which the Moon almost certainly once possessed would offer just such an event as is required to account for a considerable increase in the loss of heat by radiation at some remote period of the Moon's history.

Prof. Shaler is not accurate in his quotation of the results obtained by Langley and Very in their researches on the temperature of the lunar surface. He says (p. 6) the most trustworthy of the numerous experiments that have been made, "the series undertaken by S. P. Langley, indicate that at no time does the heat attain to the temperature of melting ice." For "heat" we should obviously read "temperature"; but the "Principal Conclusion" drawn by the authors themselves from their own research was "that the *mean* temperature of the sunlit lunar soil is much lower than has been supposed, and is most probably *not greatly above* zero centigrade" *. The italics are inserted by the present writer. The method adopted was that of determining the point at which the maximum of energy was found in the infra-red spectrum, after making allowance for the directly reflected radiation. The measured maximum was found at a point which indicated that the temperature of the lunar soil was between 0° and −20° C.; but allowance had to be made for the effect of the absorption of our atmosphere, and the most important band in this absorption spectrum was just above the measured maximum in the lunar spectrum. From the nature of the curve mapped in seemed certain that the maximum in the unabsorbed spectrum must be almost coincident in position with this band, and making allowance for this it was found that the mean temperature of the lunar sunlit soil might be as high as +50° C. But these researches have been continued by Prof. Very, who was associated with Prof. Langley in the work already referred to, and in a paper read at the Harvard Conference, 1898, Aug. 25, and printed in the *Astrophysical Journal*, vol. viii., Nov. and Dec. 1898, he finds that the mean radiation is such as would correspond to a temperature of a uniformly heated hemisphere at +97° C., whilst his measures gave for the subsolar point a temperature of 454° absolute, or 181° C.; he concludes that "only the most terrible of Earth's deserts, where the burning sands blister the skin, and men, beasts, and birds drop dead, can approach a noontide on the cloudless surface of our satellite" †. Prof. Shaler has probably taken his account of Langley's result from the introduction to Elger's book, with which he seems familiar.

He takes an exaggerated view of what may be done with large telescopes when he says:—"With the best modern telescopes, under the most suitable conditions of observation, the Moon is seen as it would be by the unaided eye if it were not more than about 40 miles from the observer." And he explains that the conditions of seeing are more favourable when we examine the Moon, because we have less atmosphere interposed than when we examine a mountain-range at a distance of 40 miles. He thinks that many of the features are observed "about as well as we could discern them by the naked eye if the sphere were no more than 20 miles away." That less atmosphere is interposed is true, but astronomers sigh in vain for conditions of that atmosphere that would permit the profitable employment of a power of 6000 on the Moon. We are reminded of the sensational claims made for the Paris telescope.

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* Am. Journ. Sc. vol. xxxviii. p. 440.

† *Loc. cit.* p. 286.

The book is well printed, but it contains many mistakes that should have been corrected by a careful editor. The note at the bottom of p. 5 is incorrect. The east side of the Moon is that which is nearer to our own eastern horizon as we see the Moon when on the meridian, and so it comes on the right-hand side of the lunar map and not "where it is customary to place the west on terrestrial maps." Prof. Shaler in several places speaks of the "heat" of a body where he means its "temperature." On p. 17 he talks of the conversion of "momentum" into heat, and on p. 62 assumes the effects of impact proportional to relative velocity instead of to the square of that velocity. His use of lunar names is most puzzling. When on p. 20 he talks of the Mare Nectano it is not difficult from the context to discover that he means the M. Serenitatis. Thibet is easily corrected to Thebit on pp. 13 and 41. The puzzle comes when the names employed, or something like them, belong to regions which are certainly not referred to. Thus on p. 13 Vendalinus (*sic*) should probably be Blancanus, on p. 20 the Mare Humorum may be corrected to the M. Nectaris, on p. 34 M. Nubium should be M. Vaporum, on p. 40 M. Crisium may denote M. Nectaris. The transformation on p. 44 of M. Gaudibert's name into Gandilot may have been effected by the printer. This unfortunate use of wrong names leaves the writer doubtful whether the statement on p. 32, that the floor of Grimaldi may owe its colour to infusion of lava from a neighbouring mare, is really intended to contradict that on p. 18, that the resemblance cannot be thus explained, or whether Prof. Shaler may not have had some other formation in his mind. Several of the references to the Plates are incorrect, and many more references might have been inserted with advantage.

The Plates themselves at the end of the book are excellent reproductions of some of the best modern photographs, but in the letterpress which accompanies them the same want of careful editing is only too manifest. In the description of Plate III. the statement is made that "the larger of the two dark pits near the northern end of the Apennines is Eudoxus, the smaller Aristotle." Aristotle is really larger than Eudoxus, and the two are near the north end of the Caucasus. In the same description references are made to Eratosthenes, which is quite invisible, and to Archimedes, of which the barest trace is just discernible. In that of Plate VI. it is stated that Grimaldi "has a floor nearly as dark as the seas," and it is implied that it is seen under a nearly vertical light. Its colour is visually much darker than that of the seas, and on the photograph it is only just within the terminator. The crater described under Plate VII. as Playfair is really Clavius. The source from which Plate IX. is derived is not stated, but there was probably a mirror somewhere in the optical train, as the western part of the Moon appears on the right-hand side of the picture. In the description the terms eastern and western must be interchanged. The area depicted is described as from "near the Moon's equator to near the south pole"; really the bottom of the plate is at about 28° S. lat. In Plate XII. the description of another rill, near to that of Hyginus, would seem to refer to the end of the Ariadæus rill, which, however, does not at all obviously "intersect a vulcanoid." The "large crater north of Hyginus" can only be understood as referring to Triesnecker, which is S.E. of Hyginus. Plate XIII. is described as extending to about 55° S. lat. (it really extends only to 35° S. lat.) and from "the polar axis westwardly"; but surely to photograph the polar axis would require an application of Röntgen Rays beyond the skill even of Mr. Ritchey; the central meridian is doubtless intended. Under Plate XVII. we are told to compare objects near the terminator with others over which "the Sun is more than 45° above the horizon." The terminator is at about 35° W. long. and the margin of the plate at about 5° W. long.; the altitude of the Sun can therefore never exceed 30°. Under Plate XVIII., which would seem to be taken from the same negative as Plate XXII., reference is made to the crater Jansen, which is situated beyond the margin. The dates of Plates XX., XXI., XXIII. should be 1901, Nov. 20, and not Nov. 21; that of Plate XXII. 1901, Aug. 4, and not Aug. 3. In the descriptions of many of the plates the age of the Moon is given in days and hours; for any selenographic purpose this is quite useless; what is really required is the longitude of the terminator or of the subsolar point, and this may vary within wide limits for Moons whose age is measured by the same number of hours. The difficulty was apparently felt in the descriptions of Plates III. and IV., where "the difference in the position of the terminator" is explained as due to the fact that "the pictures were taken at different times of year." This would seem to imply that the libration is a function of the time of year, unless the "position of the terminator" is to be taken as referring to its inclination to the margin of the plate.

Other inaccuracies might be noted in these descriptions, but enough has been said to justify the criticism offered. If it is thought that too much stress has been laid upon defects, the writer would reply, first, that many of these are of a kind which ought not to be found to such an extent in publications ranking as high as the 'Smithsonian Contributions to Knowledge' deservedly do, and, secondly, that criticism is invited in the interests of the more extended treatise which is to follow. Selenographers will look forward to the appearance of this treatise with the greatest interest, and the writer hopes that their appreciation of the value of the work which Prof. Shaler has done will not be sensibly diminished by minor blemishes in the form of its presentation.

S. A. Saunder.

January Fireballs.

The note in the *Observatory* (February, p. 114) indicated that the epoch January 9–13 was pretty well supported this year by the apparition of fireballs on January 9 and 14. Of the latter, three observations were received, and these show that the radiant was in Monoceros, at about 119° +3°, and that the height of the meteor was from 60 to 29 miles over Brecon to Aberystwith, in Wales.

The epoch of January 28, referred to in *Observatory* (January, p. 52), has also been amply corroborated this year by the appearance of two fireballs on January 27 (5^h 55^m and 11^h 58^m) and one on January 29 (6^h 40^m). Five observations of the one on January 27 11^h 58^m have come to hand, and though the data are not complete and not quite consistent one with another, the radiant is shown to have been at about the same point as that of the meteor of January 14 10^h 15^m. The height of the object was from 66 to 33 miles over the English Channel south of Portland Bill, Dorset. The meteor was a very brilliant one. Mr. G. F. Oldham, of Tunbridge Wells, says that it lasted 4 or 5 seconds, and that its aspect towards the end of its flight was that of a chain of beads, the terminal one flashing so brightly as to illumine the landscape like the Moon at first quarter. Mr. E. M. Nelson, Beckington, near Frome, describes the object as lighting the road up. It was stationary in a position about 25° N.E. of Sirius, and expanded greatly as it burst, some red fragments finally dropping from it. Mr. D. E. Packer, Selly Oak, Birmingham, writes that the meteor illuminated the sky like the full Moon, and that the maximum splendour endured 5 or 6 seconds. The object was apparently stationary at 118° −18°, but at the end there was a slight descent southward. Mr. R. Bevan, of Pontypridd, alludes to the meteor as creating a magnificent effect. It was of a bluish colour, burst like a rocket, and remained visible for 10 seconds in the "South heavens, travelling south-east." Mr. J. Ryan, of Kensal Green, N.W., noted the path as beginning 3° below κ Orionis and ending same distance below β. From 1st mag. the meteor increased in size until it burst into a brilliant green ball. Duration 8 seconds.

Of the large meteors of January 27 5^h 55^m and January 29 6^h 40^m, very few observations appear to have been made. The latter object was very brilliant. Mr. A. F. Parbury, of Haslemere, Surrey, estimated it as 3 or 4 times exceeding the lustre of Venus, He says the path was a decided curve, a few degrees below the planet Venus. The meteor was seen by an observer at Bristol, who, noticing a bright illumination of the sky, looked up and noticed the fireball descending to the right of Orion, but the exact course was not recorded.

Bristol, 1905, Feb. 9. W. F. DENNING.

P.S.—In February large fireballs were recorded on February 11 at $10^h\ 10^m$, $10^h\ 22^m$, $10^h\ 30^m$, and $11^h\ 45^m$ P.M.; February 13, $7^h\ 59^m$ P.M.; and February 18, $7^h\ 15^m$ A.M. (in daylight).

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Annular Eclipse of A.D. 1263.

GENTLEMEN,—

Haco IV., King of Norway, made an expedition against Scotland in the year 1263 to secure his possession of the Hebrides, which was threatened by Alexander III. His fleet sailed, we are told in the Norse Chronicle, on the 15th of July, and anchored for some time at Ronaldsvo, in Orkney. Whilst there, on the 5th of August, according to Johnstone's translation of the Norwegian account of the expedition (Goldsmid's edition, p. 30), "a great darkness drew over the Sun, so that only a little ring was bright round his orb."

Tytler remarks in his 'History of Scotland' (Eadie's edition, vol. i. p. 63) that calculation shows that an eclipse of the Sun on that day was annular in the Orkneys, and that this fact settles the date of the expedition, and proves that the Melrose Chronicle was in error in assigning A.D. 1262 for it. Chambers's 'Encyclopædia' (vol. iii. p. 228, under "Chronology") also states that the eclipse was annular there, and gives Sir David Brewster as its authority for this. It would seem, however, that such was not really the case. According to Oppolzer's 'Canon der Finsternisse,' the central line passed from North-west America over the Arctic Ocean (passing within 10° of the north pole), then crossed Norway and Sweden (the latter near Stockholm), the Baltic, Poland, and the Black Sea, and ended near the Lower Euphrates. But although it was considerably to the north of the Orkneys, the eclipse must have been very large there; and it seems to me (though I hope this letter will induce Dr. Dreyer to give us his view on the point) that we may really consider this phenomenon as a confirmation of the date of an historical event. With Haco's subsequent defeat in October at Largs, in Ayrshire, and his death at Orkney during his return from the ill-fated expedition, we need not concern ourselves here.

Yours faithfully,

Blackheath, 1905, Jan. 7. W. T. LYNN.

A Bibliography of Halley's Comet.

GENTLEMEN,—

Your readers will, ere this, have become aware of the prize of one thousand marks offered by the Astronomische Gesellschaft "for the best determination of the positions of Halley's Comet for the year of its return," the announcement having been made in a recent number of the 'Vierteljahrsschrift der Astronomische Gesellschaft.'

I am collecting notices for a manuscript entitled "Halley's Comet: its Past History and 1910 Return; a Short Bibliography,' which will probably be published during 1905. This collection, which is by no means intended as an exhaustive one, has been undertaken as a natural sequence to my bibliographical notes on Dr. Halley's life and labours, printed in the London *Notes and Queries*, 1902–3–4.

Any bibliographical notices pertaining to the history of Halley's Comet, and especially to its 1910 return, will be gratefully received by

Yours very truly,
EUGENE F. MCPIKE,
Member of the Institut International de Bibliographie, Brussels.

1 Park Row, Chicago, U.S.A.,
1905, Feb. 8.

The Lunar Eclipse.

GENTLEMEN,—

A fine sky favoured me for the observation of the partial lunar eclipse of the 19th instant. I noticed as especially remarkable the brightness of the shadow. The eclipsed part of the Moon was plainly seen with the naked eye, even since the beginning. The rosy hue was evident, but perhaps less strongly than as usual. With a 2-inch O.G. refractor, I saw in the shadowed part some features of the lunar topography; Aristarchus was shining in the dark as a little star. The eclipse was a bright one, a striking contrast with that of the 11th April 1903. Our atmosphere must have been very transparent and free of dust.

I remain, Gentlemen,
Yours faithfully,
M. MOYE.

Montpellier (Hérault), France.
1905, Feb. 20.

[We have a similar communication from Mr. S. C. Russell, of Epsom, who remarks, as do several other persons, that the eclipse was not a dark one, and the colour a subdued slate-grey. The appearance of Aristarchus as a bright point in the dark shadow seems to have been a noticeable feature of this eclipse.—EDS.]

Periodical Comets due in 1905.

GENTLEMEN,—

There is a somewhat melancholy satisfaction in referring (as Mr. Denning does) to Prof. Barnard's comet of 1892 (the fifth of that year), it being, I believe, the last, with one exception *, the orbit of which was calculated by Dr. Hind, who died on the 23rd of December, 1895. Hind's elements are given in *Ast. Nach.* No. 3271 (vol. cxxxvii. col. 109). According to them, another perihelion passage was due in the summer of 1899, and the next not until about February 1906. He remarks on the difference between his own result and that of others, which seems to show that no very accurate determination was possible from the observations at that one appearance, but points out the near agreement between his places and those observed at Vienna from October 16 to November 13. At any rate it could hardly be included amongst comets due in 1905.

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* Comet 1894 I., discovered by Mr. Denning on March 26.

As regards Tempel's first periodical comet, discovered in 1867, as it has not been seen since 1879, and did not return even in 1892, after M. Gautier's calculation of its lengthened period, it seemed to me that we should henceforth have to regard it, like Biela's, as a departed friend, and that without any hope of seeing meteors which may be connected with it, because its perihelion distance is so great (about 1·7). Yours faithfully,

Blackheath, 1905, Feb. 8. W. T. LYNN.

OBSERVATORIES.

PERTH, WEST AUSTRALIA *.—Steady progress has been made, but there is less tangible result than usual. Towards the end of 1903 it was realized that the transit work was being pushed forward rather too vigorously for the size of the staff, and although reductions were pretty well up to date there had been no time to check them. It was decided, therefore, to drop the zone work with the transit-circle entirely for one year and to check the computations. This work has been completed, but the figures are not yet entered in the star ledger. Meanwhile, advantage was taken of the comparative rest of the instrument to investigate the errors of division, and it was found, as had been anticipated, that the graduation was very faulty in just those portions where readings for the zone-stars occurred.

With the assistance of a movable circle and a microscope with a divided object-glass, the error for the mean of four microscopes has been determined for each degree of pointer-reading, and also for each division (5) between 359° and 10°. There is one very bad spot, as will be seen by the following errors, which are for the mean of four microscopes:—

Pointer.	Error.	Pointer.	Error.
° ′	″	° ′	″
6 35 ……	−0·49	6 50 ……	+3·41
6 40 ……	+0·08	6 55 ……	+4·18
6 45 ……	+1·12	7 00 ……	+4·64

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* We are indebted to Mr. W. E. Cooke, the Director, for this account of the work of his Observatory during the year 1904.

It is quite evident that the time spent upon division-errors has not been wasted.

A number of observations were made by the two principal transit observers (Messrs. Joscelyne and Curlewis) for determination of relative personal error as between facing south and facing north. The zone-stars are all south of the zenith, whereas clock-stars are north. The following errors were determined and will be applied to the zone-star positions:—

J.	$-0^{s}{\cdot}05$
Cu.	$-0{\cdot}01$

There is no doubt that some correction for magnitude is required, and it was my intention to investigate this during the year, but as the result of some correspondence with Prof. Pickering, of Harvard, it was decided to drop the investigation for the present.

A number of stars north and south of the zenith were observed as a check upon the value of latitude deduced from circumpolars, and another value was also obtained from clock-stars observed north of the zenith. The results are:—

From circumpolars	−31° 57′ 7″·40
„ N. and S. stars......	7 ·50
„ Clock-stars	7 ·65

The value to be adopted from 1905, January 1st, will be 31° 57′ 7″·4. The difference between this and the former value (31° 57′ 9″·6) is due to errors of division. No allowance has been made for variation of latitude, but the circumpolar observations extend over several years.

The Observatory has co-operated with the Lands Department in fixing the astronomical position of a few places. The almucanter method of determining time and latitude with a 12-inch theodolite described in 'Monthly Notices R. A. S.' for January, 1903, has been adopted, and has been found to work very successfully. Some very interesting discrepancies between astronomical and geodetic positions point to irregular local disturbances of the vertical, and will require further investigation.

Mr. Johns, who was in charge of the astrographic telescope, resigned on May 31st, and Mr. Nossiter, who had already taken some observations with that instrument, was appointed in his place. The taking of the catalogue plates has therefore not been interfered with, and 515 were passed as satisfactory during the year, making a total of 742 in the zones of even degrees of declination, and 132 in the odd zones completed. Unfortunately no provision has been made for measuring them.

If the State Government remains in its present mood with respect to astronomical work, the only thing will be to prepare the catalogue piecemeal. There is no likelihood of taking any measures during 1905, but we may reasonably hope to finish the plates by next December. Then the telescope will probably be allowed to remain idle whilst Mr. Nossiter starts measuring with what assistance he can get from the meteorological staff.

NATAL.—The Report of this Observatory for the year 1903 has lately come to hand. It seems that the staff underwent considerable reorganization during the year, for the post of Chief Assistant was filled by the appointment of Mr. Rendell (from Greenwich) in April 1903, and a month later Mr. A. E. Hodgson, formerly of the Solar Physics Observatory, South Kensington, took over the duties of Junior Assistant. Mr. F. A. Hammond remains the Meteorological Assistant. The work of the Observatory is still mainly meteorological, but arrangements have been made to obtain some apparatus in order to include astronomical photography in the work of the Observatory. Borrelly's Comet of 1903 was observed by means of a cross-bar micrometer. and an orbit deduced from the observations which agrees with other results. Transit observations are made for determination of time.

PUBLICATIONS.

ASTRONOMICAL DISCOVERY*.—This book is a review of the more striking astronomical discoveries of the last two centuries, and an attempt to draw from them lessons for the future. It follows of necessity that it contains many oft-told stories, with, however, the addition of many interesting details concerning them that are not to be found in most of the text-books.

The discovery of Uranus comes first, and is shown to have been no mere accident, but the result of a combination of a keen and trained eye, alert attention, and perseverance in carrying out a large programme.

The next section deals with the discovery of the minor planets, culminating in that of Eros. The lesson drawn is, "Just when we are on the point of relinquishing (some line of research) under the impression that the mine is exhausted, we may find a nugget worth all our previous and future labour."

We next come to the discovery of Neptune, which is described in great detail. The question of the relative blame attaching to Airy and Challis is discussed, the verdict being given against the latter, though, as the author admits, opinion differs widely on the point. Science, and especially astronomy, has now become so cosmopolitan that much less interest attaches to these questions than formerly. We are content to rejoice that Neptune was finally discovered, without wasting time on international jealousy or vain recrimination.

The element of luck in Galle's success is that Neptune was only 1½° inside the limit of the Berlin star map. Had it been beyond this, it is very probable that Challis would have been first in the race.

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* By Herbert Hall Turner, F.R.S., D.Sc., Savilian Professor of Astronomy in the University of Oxford. London: Edward Arnold. Price 10s. 6d.

The chapter on Bradley is perhaps the most interesting in the book. There are so many personal incidents which help us to realize his career, his friends, and the workings of his mind, which led up to his brilliant discoveries of aberration and nutation. It is satisfactory to read how highly his work was appreciated at Oxford, the Professor of Poetry saying of him, in the Sheldonian Theatre, "What men of learning! what mathematicians! we owe to Savile ...... one whom I will not name, for posterity will ever have his name on its lips."

Under the heading "Accidental Discoveries" we have an account of the Astrographic Chart of the heavens, the idea of which was suggested by the photographs of the great Comet of 1882 at the Cape. This leads up to the discovery of Nova Geminorum at Oxford. This subject and the Variation of Latitude have been already fully dealt with in the columns of the *Observatory*, so it suffices to note that the book contains interesting details of the former and a full account of Chandler's great discovery.

In the sun-spot chapter a diagram is given which appeared originally in this magazine *. Its object is to show that a connection between the solar cycle and meteorological phenomena cannot be safely inferred from a short period of comparison. From 1858 the curve of excess of retrograde turns of the Osler Anemometer at Greenwich over direct turns shows close accord with the curve of Wolf's sun-spot numbers. However, on extending comparison before and after, the resemblance is lost. The conclusion is that this and many similar examples of alleged connection between sun-spots and weather are merely fortuitous, and would disappear if a longer period were examined.

The general conclusion of the book is that there is no royal road to discovery. Practically the only rule is "that discoveries are seldom made without both hard work and conspicuous ability." A mixture of caution and daring is required in the discoverer, and the genius to perceive when an apparently barren research may still be profitably continued, and when, on the other hand, it is better to break it off and start a new line.

It is to be hoped that this record of past triumphs may stimulate many to renewed exertions in their chosen field, in the hope of themselves making some striking addition to the sum of human knowledge.

'Remarkable Comets' and 'Remarkable Eclipses' (Sampson Low, Marston & Co.).—Mr. Lynn has just published new editions of these handy little works. For evidence that they are brought up to date we may refer to the fact that the recent appearances of Encke's Comet and Tempel's second periodical comet are mentioned. And we may take this opportunity of quoting what Prof. Lebon says of them in his 'Plan d'une Bibliographie analytique des écrits contemporains sur l'Histoire de l'Astronomie,' laid before the International Congress of Historical Sciences at Rome in 1903 :—

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* 1889 June.

> M. W. T. Lynn, dans de nombreuses Lettres à l'Editeur de *The Observatory*, a signalé des rectifications diverses à apporter dans l'Histoire de l'Astronomie, depuis l'Antiquité jusqu'aux temps Modernes. Notamment, il donne les résultats de ses recherches sur les époques d'apparition de comètes et d'éclipses ; il prouve que c'est bien Dominique Cassini qui a le premier découvert la principale division de l'Anneau de Saturne ; il relève l'inexactitude de détails relatifs à la vie et aux travaux de quelques astronomes. M. W. T. Lynn a résumé les résultats de ses recherches dans deux Opuscules sur les comètes et les éclipses remarquables.

NOTES.

Comet Notes.—Mr. Chas. Lane Poor has produced a most interesting work on Brooks' Periodic Comet of 1889, 1896, 1903. He was aided in his computations by several ladies, members of the computing staff at Columbia University. Perturbations by Venus, Earth, Mars, Jupiter, Saturn have been carefully applied. The most interesting part of the discussion is the very close approach which the comet made to Jupiter, 1886 July 20, when it passed only 55,000 miles from its surface. An actual collision with Satellite V. is quite possible, but not certain, and it is probable that the splitting of the comet into two portions dates from this time. We are not surprised to find that such a close approach utterly changed the orbit of the comet. Before the approach its perihelion was on Jupiter's orbit, its aphelion far beyond Saturn's orbit, its period about 29·2 years (31·5 was first found, but when Jupiter's oblateness was allowed for it became 29·2). After the approach its period was 7·09 years, its aphelion near Jupiter's orbit, its perihelion somewhat outside that of Mars.

The question of identity with Lexell's lost comet of 1770 (which made a close approach to Jupiter in 1779 and was not afterwards seen) is then discussed. It is not absolutely impossible that the two are identical, but this could only be the case if we suppose another close approach to Jupiter in 1827, and to Saturn about 1790. It is considered more probable that Swift's Comet (1895 II.) is identical with Lexell's, but in neither case is a definite answer possible.

The following are the final elements for the last three apparitions :—

Epoch in G.M.T	1889 Sept. 30·5	1896 Oct. 23·5	1903 Sept. 18·5
Equinox..................	1890·0	1896·0	1903·0
μ........................	$501''\cdot6807$	$499''\cdot9702$	$499''\cdot7400$
L	$1^\circ\ 36'\ 28''$	$0^\circ\ 11'\ 51''$	$350^\circ\ 48'\ 40''$
π	1 35 8	1 48 57	1 45 8
☊	17 59 1	18 1 4	18 6 24
ϕ	28 5 12	27 59 59	28 1 1
i	6 4 6	6 3 33	6 3 45
M_0	0 1 19	358 22 54	349 3 31

The following are the elements in 1883, before the close approach to Jupiter:—

Epoch	1883 Feb. 8·5.		
T	1886 July 24·06.		
Equinox	1886·0		
μ	121″·4483		
L	146°	6′	28″
π	188	40	2
☊	186	57	41
ϕ	24	56	11
i	6	18	3
M_0	−42	33	33
Period	29·22 years.		

The following elements of Comet 1904 II. (Giacobini) are by Prof. Aitken from his observations of Dec. 19, 27, Jan. 9:—

T........	1904 Nov. 3·227 G.M.T.			
ω	40°	42′	35″	1905·0.
☊	218	28	4	
i	99	36	41	
log q	0·27454			

Borrelly's Comet (1904 *e*) proves to belong to Jupiter's family; the following elements have been deduced by M. Fayet from a large number of observations extending from the end of December to Jan. 26:—

T	1905 Jan. 16·6537 Paris M.T.			
ω	352°	13′	59″	1905·0.
☊	76	41	34	
i	30	31	59	
log q	0·145175			
log e	9·79221			
μ	503″·932			
Period	7·042 years.			

Almost identical elements have been deduced by Aitken and Wedemayer. Their periods are 7·30 years, 7·518 years respectively.

The elements show a distant resemblance to those of the comet of 1783.

Ephemerides of the two comets (for Berlin midnight) follow:—

	1904 II.					1904 *e*.				
	R.A.			N. Dec.		R.A.			N. Dec.	
	h	m	s	°	′	h	m	s	°	′
Mar. 1......	21	3	10	61	29	3	22	3	32	0
5......	21	24	12	62	20	3	33	44	33	47
9......	21	45	6	63	3	3	45	49	35	25
13......	22	5	41	63	38	3	58	16	36	56
17......	22	25	47	64	6	4	11	6	38	19
21......	22	45	14	64	27	4	24	16	39	34
25......	23	3	52	64	43	4	37	45	40	42
29......	23	21	31	64	56	4	51	31	41	42

Ast. Nach. 3997 contains some measures of the brightness of Encke's Comet at the recent apparition by J. Holetschek. The combined light of the object was equivalent to that of a 9th magnitude star on Nov. 25, a 6th magnitude on Dec. 10, a 5th magnitude star on Dec. 27. A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Mag.
PW	1905 Jan. 8	Götz.	—
PX	26	Wolf.	11·6
PY	14	,,	13·3
PZ	Feb. 9	,,	13·5
QA	9	,,	13·5
QB	9	,,	12·0
QC	Jan. 14	,,	13·2

PW was at first mistaken for 517; either PY or QC may possibly be identical with 427. PV, which was found in January near Jupiter, proves not to be new, but identical with 149 Medusa. PX, QB are probably identical with 180 Garumna, 517 respectively.

The following planets have received permanent numbers:—NC 522, ND 523, NN 524, NO 525, NQ 526, NR 527, NS 528, NT 529, NV 530, NW 531, NY 532, NZ 533, OA 534, OC 535, OF 536, OG 537, OK 538, OL 539, ON 540, OO 541, OQ 542, OT 543, OU 544, OY 545, PA 546, PB 547, PC 548.

The following planets do not receive permanent numbers, not having been sufficiently observed:—NE, NF, NG, NJ, NK, NL, NM, NU, NX (this seems to have a small mean distance), OB, OD, OE, OH (this may be 353), OJ (this may be 411), OP (this may be 398), OR (this may be 465), OS, OV, OX, OZ, PD, PF.

The following identities have been established:—NH with 200, NP with 255, OM with 236, OW with 485, PE with 178.

Planet 496 has been named Gryphia. A. C. D. C.

JUPITER'S SIXTH SATELLITE.—Some further information regarding this body has been received from Lick. It passed elongation on December 25 at distance 50′, and has been observed by Mr. Aitken with the 36-inch refractor. Prof. Kreutz has written pointing out that the correct distance from Jupiter on January 4 was 45′, not 49′ as was given last month on the authority of the *Astronomical Journal*. Making this alteration, a circular orbit was deduced, which gave distance from Jupiter 6 millions of miles, period 186 days, rate of approach to Jupiter on January 4 20″ daily. This last is not in good accord with the 45″ telegraphed, so that the orbit would seem to deviate largely from a circle. The facts so far published are quite consistent with the hypothesis of the object being a minor planet, but the December positions, when published, should suffice to settle the question.

Prof. Young writes in *Nature* for February 16 that he considers that the term "apparent motion retrograde" refers to the orbital motion. It seems, however, far more likely that it refers to the diminishing position-angle. Observations do not yet extend over a long enough period to settle the direction of orbital motion, and nothing can be safely deduced from comparison with the other satellites, since the plane of its orbit is evidently quite different from theirs.

A SEVENTH SATELLITE OF JUPITER.—Just as we are going to press news comes that still another satellite has been found. The Kiel telegram says, "On February 25 a seventh satellite was seen, of magnitude 16, position-angle 62°, distance 21′, motion south-east." Neither the name of the discoverer nor the observatory was given, but from a Reuter's telegram in the daily papers this appears to be another discovery by Prof. Perrine. The very latest news says that it has been under observation with the Crossley Reflector since January 2.

THE MEETING OF THE BRITISH ASSOCIATION IN SOUTH AFRICA.—We learn from a copy of the *Johannesburg Star* that Sir David Gill has lately been in that town to help in the formation of a Committee to organize the reception of the members of the British Association who will spend a week in Johannesburg from August 28 next. It appears from the account that papers in each of the eleven sections which the work of the Association covers will be read during the visit. Mr. A. R. Hinks will lecture on an astronomical subject, Prof. C. V. Boys will lecture on some subject in physics, and Prof. G. H. Darwin, the President of the Association for this year, will deliver the Presidential Address in Johannesburg. It is estimated that about 200 or more persons will come to the town, and that the cost of their reception will be about £6000. The preliminary arrangements comprised the appointing of Sub-Committees to arrange (1) for private hospitality, (2) for excursions, (3) entertainments, (4) publications. Sir David Gill made a speech from which the following passages are extracted:—

> He had been asked to give them a short account of how this invitation had come about. When he was in London in 1900, he was approached by the Council of the British Association to attend a meeting at which to lay before them what were the possibilities of a visit to South Africa. He told them the possibilities were impossible as long as the war continued, but that as soon as the war was over they would communicate on the subject. As soon as things were fairly settled, he addressed the various Governments of South Africa, and told them that the British Association had said so far that "Barkis is willin'" to come, and asked what the Governments would do to aid in bringing them here. As he had said on the previous day, men of science were not very wealthy men as a rule, and it was deemed necessary for the members of the British Association to have some aid towards their expenses in coming out. The various Governments had responded in a most generous way, the Cape Government offering £3000, the Transvaal and Orange River Colony £2000,

and Natal £1000, so that £6000 is at the disposal of the British Association towards assisting those they desired to help in the matter of their passage.

A number of men, some five or six, were coming out several months in advance of the Association, and would make special studies in South Africa in connection with their own specialities, and would in their inaugural addresses as presidents of sections, or in the form of communications or lectures, indicate the results of their special investigations in Africa to the meeting. He thought that was a point that would be appreciated. They had already in their hands a list of the men who were coming, and had heard what lectures were to be delivered. If they read the list they would find how extremely representative it was. There was no branch of known science that was not represented—in many cases by men who were the very leaders in those departments of science, and he (the speaker) knew full well the list would be largely added to. A great many men had been waiting for information as to what it would cost them to come, and he must say that the Government Railways of South Africa had met them in an extremely liberal manner.

THE OPENING OF THE TRANSVAAL OBSERVATORY.—The speech on the previous day referred to by Sir David Gill in the extract printed above was made by him at the formal inauguration by Lord Milner of the Meteorological Observatory of the Transvaal at Bellevue, near Johannesburg, on February 17. Mr. R. T. A. Innes, the Director, sketched briefly the history of meteorological research in our new colony, which, it seems, owes much to Mr. Lyall Soutter, of Pretoria, who somehow obtained possession in 1891 of some instruments which had been bought but never used by the late Government, and kept a systematic record of rainfall. This, with a few more rainfall-records made in connection with the development of the mines, was all the meteorology practised before the war; but since then the Government Meteorological Department, with Mr. Innes at its head and a large body of voluntary observers, of whom Mr. Soutter is one, at scattered stations, has been established.

Sir David Gill referred to Mr. Innes as an unfortunate man, in having to spend his time in the dull routine of meteorology when his heart was so much in astronomy, and hinted at a not very distant date when an astronomical observatory might be attached. Lord Milner seemed to think there might be a difficulty about finding the money.

RECENT SUN-SPOTS.—The spot whose appearance on January 29 was briefly recorded last month turned out to be the largest that has appeared on the Sun since the great spot of 1892 February, as its area measured more than three thousand millionths of the Sun's visible disc. It crossed the central meridian on February 3, its centre being on the meridian about midnight of that day; and it may be noted that there was a "moderate" magnetic disturbance at Greenwich on that day, beginning about an hour and a half after midnight of February 2 and reaching its maximum about the following midnight. The spot, whose heliographic coordinates were approximately long. 330°, lat. −16°, decreased in size after passing the central meridian, left the visible disc on February 11, and was seen again on a photograph taken at Greenwich on February 25 well within the eastern limb. Many other spots have been seen during the month; on February 11 as many as eighteen groups were counted. Father Cortie sends an account of an interesting observation:—

> Yesterday I got a small sun-spot in the spectroscope almost on the limb of the Sun. There was a beautiful prominence over it on the C line, and the roots of the prominence ran right down to the spot, projected on the Sun's disc; when it reached the spot the line was again brilliantly reversed, like a star, over the spot, evidently corresponding to the bright faculæ surrounding the spot as shown on our drawing of the morning. D_3 and F also showed the prominence very brilliantly, but I could not detect any other reversals. The spot was the little N. spot preceding the fine double spot almost on the W. limb. On opening the slit wide on the F line over the two members of the group, I could distinctly see on the disc local brightenings due to the hydrogen flocculi over the spots.

The Total Solar Eclipse of August 30.—The following arrangements of observing parties is published in *Nature* of February 23, partly on the authority of the Secretary of the Joint Permanent Eclipse Committee:—

Labrador . .	Lick Observers.	
Spain	Mr. Evershed.	Near Burgos.
	Prof. Callendar.	Near Oropesa.
	Prof. A. Fowler.	
	Mr. W. Shackleton.	
	Lick Observers.	
Algeria . .	Sir Norman Lockyer.	Near Philippeville.
	Dr. W. J. S. Lockyer.	
	Mr. C. P. Butler.	
	Mr. H. F. Newall.	Near Bona.
Tunis	The Astronomer Royal.	Sfax.
	Mr. Dyson.	
	Mr. Davidson.	
Egypt	Prof. Turner.	
	Mr. Bellamy.	
	Lick Observers.	

Rainfall in February.—The past month has been an especially dry one The rainfall at Greenwich amounted only to 0·724 inch, which is 0·760 below the average. During the twelve months just ended, the total rainfall has been about 7 inches below the average.

In connection with Mr. Maunder's article in last month's number about a publication of Mr. Broun, it may be well to point out a mistake in Poggendorff's 'Biographisch Literarisches Wörterbuch,' which says of Broun that he was Director of Sir T. Brisbane's magnetic observatory at Makerstoun in Australia.

The facts are that he had been Director of Brisbane's observatory and afterwards observed at Makerstown in Scotland.

On the evening of March 9, Jupiter and Venus will both be in conjunction with the Moon. The planets will be separated by 5½ degrees of declination, so that their conjunction is not noted in the N.A. The Moon will be three days old on the 9th, and Venus is bright just now, attaining greatest brilliancy on the 21st, so that the configuration no doubt will be striking.

The subject selected for the Adams Prize (University Prize at Cambridge) for 1906 is "The Inequalities in the Moon's Motion due to the Direct Action of the Planets." The four examiners are the Vice-Chancellor, Prof. G. H. Darwin, Mr. F. W. Dyson, and Mr. G. B. Mathews.

We offer our congratulations to Mr. E. T. Whittaker, Secretary of the Royal Astronomical Society, on being selected by the Council of the Royal Society as one of the Candidates for election as Fellow of that body.

It is announced that M. F. J. P. Folie, late Director of the Royal Observatory at Brussels, died at Liège on January 29, in his seventy-second year.

The Royal Meteorological Society has arranged an exhibition of meteorological apparatus to be held at the Society's rooms from March 14–17.

The next Meeting of the Royal Astronomical Society will be on Friday, March 10; of the British Astronomical Association on Wednesday, March 29.

FROM AN OXFORD NOTE-BOOK.

Close on the heels of the news that Mr. John Tebbutt is giving up active astronomical work, comes the intelligence of the retirement of Sir Charles Todd at the age of 79. But in the latter case we have some consolation: it is apparently from his official work in the Post Office that the veteran is retiring; the superintendence of the Observatory he will retain; and we gather that he will also remain "punmaster-general," which anyone who has ever met him will readily believe. I hope I may without impropriety reproduce the following extract from an Australian paper:—

> Sir Charles Todd was born at Islington, England, on July 7, 1826. He joined Her Majesty's service in 1841, when he received the appointment of astronomical computer at the Greenwich Observatory. In 1847 the late Professor Challis, Cambridge University Observer, offered him the position of one of the assistant astronomers at that great seat of learning, and there the young astronomer remained for seven years. In May of 1854 the Astronomer

Royal, the late famous Sir George Airy, persuaded him to return to Greenwich as one of the assistant astronomers, especially to take charge of the new galvanic department and to supervise the regulating of the hourly time signals and the dropping of the time balls throughout the United Kingdom. There had been some difficulty with the Deal time ball, and Sir Charles went down to put it right, and witnessed on that occasion the embarkation of troops for the Crimea. While on his way back he stayed overnight at Tunbridge with a celebrated electrician, and it was there that he received a message from Sir George Airy offering him, on behalf of the Colonial Office, the position of Superintendent of Telegraphs and Government Astronomer in South Australia. He hastened back to Greenwich, and boldly confessed to his great chief that he had not been looking at sun, moon, and stars always.

"I don't know whether I can accept; at least," he added naively, "until I ask a young lady at Cambridge to say 'Yes.'"

"Off you go, sir, and see her at once," was the rejoinder of the great chief; and a message was sent from Cambridge in due course accepting the post. The interview had been satisfactory.

It is interesting to note that on his wedding day, Sir Charles Todd was prophetic as well as bashful. Replying to the toast of "The Bride and Bridegroom," he remarked that he was going out to Australia, and he hoped "he would be instrumental in bringing England and Australia into telegraphic communication."

We are every day grouping observations for discussion; and yet the following illustration (shown me by Prof. E. C. Pickering) of the important difference made according as we select one or other of two simply related quantities as the independent variable will probably come as a surprise to most of us. Suppose we have the following observed values of A and B:—

A	5	6	7	6	7	8	7	8	9
B	6	6	6	7	7	7	8	8	8

First group according to B, in sets of three for which B has the same value.

Then when $B = 6 \quad 7 \quad 8$
mean $A = 6 \quad 7 \quad 8$

Therefore $A = B$.

Secondly group according to A.

Then when $A = 5 \quad 6 \quad 7 \quad 8 \quad 9$
mean $B = 6 \quad 6\frac{1}{2} \quad 7 \quad 7\frac{1}{2} \quad 8$

Therefore $(A-7) = 2\,(B-7)$.

The following very pretty experiment emanates from Clifton:—

Two equal circular discs of thick glass are perforated at the centre so that a peg goes through them loosely. The lower one is fixed on the ground, the other placed over it, and between them are bicycle balls, so that the upper one easily rotates about the peg as a vertical axis. A boy stands on this with a pair of dumb-bells in his hands hanging down and a friend rotates him. He raises the dumb-bells to a horizontal position at arm's length and rotates slowly; drops them to his sides and rotates faster again; and so on: thus simply illustrating the accelerated rotation of a contracting planet.

The erection of the new "Monastery" on Mount Wilson, for which the Carnegie Institution have guaranteed £60,000, is a matter of such general interest that I do not scruple to extract a few sentences from letters received.

The Abbot himself writes under date January 24th:—

> I have just returned from the mountain after a delightful trip down over the face of Mt. Harvard, where we propose to run a new telephone line. I left Barnard setting up the Bruce telescope, which will be ready for work in a few days. The weather has delayed us greatly with the Snow telescope, but this also will probably be set up this week. We hoped to get at this work last week, but a heavy rain prevented the canvas men from finishing the canvas cover of the moving house.

In connection with the getting of the Bruce telescope up the mountain, an independent MS. written by the monk above mentioned, testifies to the value of the Abbot's "automobile,"—a machine which was invented for such purposes, but has little in common with the motor-car as known in Europe beyond the wheels, for its "automatic" action requires the help of two men and a donkey (called a "burro"). The MS. contains also the following practical comments on the climate:—

> The night was remarkably clear, with not a breath of air stirring. The very lowest stars that I could see showed no tremor to the naked eye. I believe that would have been a revelation with the 40-inch here.

The "Monastery" is reported most comfortable, and the monk states that if it is not exactly "good venison" that "fills his scrip," it is nevertheless the "loveliest canned goods you ever tasted."

Being asked the other day whether there was any law corresponding to Bode's for the satellites of Saturn or Jupiter, I looked in various places and found the following sentence in 'Chambers's Astronomy' (p. 67 footnote):—

> Many attempts have been made by ingenious dabblers in Astronomy to discover other arithmetical coincidences formed after the spirit of Bode's law. The following is the only one I have met with which deserves reproduction. Take the series 0, 1, 2, 4, 8, 16, 32, and 64. Add 4 to each, and the resulting figures represent with some approach to accuracy the relative distances of the satellites of Saturn from their primary.

Taking the mean distances in thousands of miles given in Young's 'General Astronomy,' and dividing them by these numbers, the quotients are as follows:—

Satellite.	Dist./Bode.		Quotient.	Diff. from mean.
Mimas	117/4	=	29·3	−1·5
Enceladus	157/5	=	31·4	+0·6
Tethys	186/6	=	31·0	+0·2
Dione	238/8	=	39·8	−1·0
Rhea	332/12	=	27·7	−3·1
Titan	771/20	=	38·6	+7·8
Hyperion	934/36	=	25·9	−4·9
Iapetus	2225/68	=	32·7	+1·9
Mean			30·8	±2·6

Now it is rather remarkable that the newly discovered satellite Phœbe has a distance corresponding, not to the next term of this series, but to the next but one. If we multiply 31,000 by 256+4, we get 8,060,000, which is approximately the number of miles in the mean distance assigned to Phœbe by the discoverer. Is there yet another satellite of Saturn at mean distance about half this? The "law" is not really a bad one, though Titan and Hyperion spoil the look of the figures considerably. For comparison with Bode, we may express the planetary distances in a unit of 1/300 of the Earth's distance from the Sun, and get quotients as follows:—

Planet.	Dist./Bode.		Quotient.	Diff. from mean.
Mercury	116/4	=	29·0	+0·3
Venus	217/7	=	31·0	+2·3
Earth	300/10	=	30·0	+1·3
Mars	457/16	=	28·6	−0·1
Jupiter	1561/52	=	30·0	+1·3
Saturn	2862/100	=	28·6	−0·1
Uranus	5755/196	=	29·4	+0·7
Neptune	9017/388	=	23·3	−5·4
Mean			28·7	±1·4

THE brilliant address delivered at the St. Louis Congress in September last by M. Poincaré has been translated by G. B. Halsted, and printed in 'The Monist' for January 1905. It is startling to find how rudely all our fundamental principles have recently been shaken—those of the conservation of energy, of the degradation of energy, of the equality of action and reaction, of the conservation of mass, and of the relativity of motion. M. Poincaré shows with wonderful clearness, and with unexpected touches of humour, how "battered" they have been lately, and speculates as to what the immediate future may have in store for us. The word "battered" is of course the translator's, and one can only conjecture the original (which I have not seen) from his general style, which is not above criticism: here are a few specimens:—

Of such an animal as exuviates, as breaks its too narrow carapace and makes itself a fresh one, under the new envelope we easily recognise the essential traits of the organism which have subsisted.

* * * * * *

The explanations proposed were numerous; but in such cases we cannot say "store is no sore."

* * * * * *

. . . . by connecting-rods or by other dispositives.

* * * * * *

Behold the ideal form of physical law: well it is the law of Newton which first covered it; and then how has one acclimated this form in physics?

Chance drew my attention recently to the opening line of Milton's "Song on May Morning," which begins:—

> Now the bright morning star, day's harbinger,
> Comes dancing from the east, and leads with her
> The flowery May.

The poem is undated, but is classed with those written about 1630. It occurred to me to enquire in what years Venus was a morning star on May 1, and rough calculation seemed to show that 1627, 1632, 1635, 1640, and 1643 are the only possibilities, of which the first is probably too early and the last two almost certainly too late. Mr. Crommelin kindly checked the calculation and arrived at the same result, although he pointed out (quite justly) that my method was defective. He remarks that the transits of 1631 and 1639 give us an easy way of getting the dates when Venus was in inferior conjunction (1631 November 26 and 1639 November 24); and hence we find the approximate dates of greatest western elongation to be as follows in *Old Style*:—

1632 February 6.
1633 September 11.
1635 April 19.
1636 November 22.
1638 July 1.
1640 February 3.

The cycle of eight years renders it unnecessary to give other years. It is not easy, however, to choose between 1632 and 1635. These years are not unlike 1899 and 1902, when the western elongations were on February 10 and April 25 respectively; and the times of rising of Venus for Greenwich on May 1 are given in *The Companion* as $15^h\ 39^m$ and $15^h\ 16^m$ respectively, that of sunrise being $16^h\ 33^m$. There remains, of course, the question whether Milton wrote from observation at all, or merely considered that the morning star ought to be there on May morning anyhow.

From the "Agony" column of the *Times* on February 13 last:—

SEASONABLE PRESENT. The celebrated "ASQUITH" TELESCOPE. For Seeing "Invisible Exports," star-gazing, &c. Useful to unemployed looking for work; or at Foreign Aliens (who are best seen at a distance). This marvellous telescope will make an object appear Black or White as desired. Enables the difference between Trades Unionism and Protection to be distinguished. Highest testimonials from Young Men of Vast Commercial Experience, gained at Oldham, Carnarvon, Greenwich, &c.—Herr Schmidt (see below), 29a, Victoria-street, Sole Agent for Weltgrabber, Berlin.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1905 March 10.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: E. T. WHITTAKER, M.A., and
T. LEWIS.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Lewis. Since the last Meeting we have received 90 presents. Calling for special mention are a further series of enlargements of Astrographic Chart Plates from the Royal Observatory, Greenwich; Six volumes of Transit-circle Observations, in MSS., made at the Madras Observatory, presented by the India Office; and some Stereoscopic Views of the Stars, presented by Mr. T. E. Heath.

The President. Before we proceed to the reading of the papers, I have an announcement to make which I think the Fellows will like to hear. As you know, during the Presidency of Prof. Turner we had some very successful extra evening meetings. It is proposed to continue these meetings, and I am glad to say we have persuaded Prof. Turner to give us a lecture in April or May—the exact date and title have yet to be fixed *—on the lines of research which are most desirable to be adopted in the forthcoming Solar Eclipse, and the best instruments for carrying out the observations. The first paper to be taken to-night is one by Dr. Lockyer, on the "Spectroheliograph of the Solar Physics Observatory."

Dr. W. J. S. Lockyer. At the Solar Physics Observatory experiments have been carried on since 1897 in order to produce pictures by monochromatic light. The present instrument, which I am about to describe, is the result chiefly of these experiments. Prof. Turner so recently described the principle of the spectroheliograph, that this need not be referred to again, but I will pass immediately to the form in use at South Kensington. The most ideal form of instrument is undoubtedly one where the solar image is stationary and the spectroheliograph itself moves. The advantage of this is that the entire motion in the spectroheliograph is one in a horizontal direction, and any shaking due to the moving parts does not affect the image. Prof. Hale adopts the principle of keeping the spectroheliograph constantly fixed relatively to the telescope, the latter being mounted equatorially, and he moves the telescope in right ascension and the photographic plate at the same time and speed, and in that way produces his monochromatic images. At Potsdam there is an instrument which is on rather a small scale: in this case the whole spectroheliograph is moved in relation to the focussed image: from all accounts they secure very good pictures. In the first place, I will just show you, by means of some slides, the general arrangement of the instrument as employed at South Kensington.

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* [It has since been arranged that this lecture shall be given on Wednesday, May 3, at 8^h 30^m P.M.—EDS.]

Dr. Lockyer went on to describe the apparatus, which consists of a siderostat made by Messrs. Cooke of York, with slow motions in R.A. and declination, actuated by electric motors, so that the observer, though 30 or 40 feet away, has perfect control of the instrument. The mirror has an aperture of 18 inches. The image of the Sun is formed on the slit by a Taylor lens of 12 inches diameter, mounted on a mahogany stand on a concrete column. The diameter of the image is about 2¼ inches, but this varies in size, on account of the great changes of focal length of the lens with temperature. This focal length does not appear to change with the temperature of the room, but alters when the solar beam is thrown on the lens, the variation in ten minutes being sometimes as much as a quarter of an inch. Photographs taken with the apparatus on the calcium line were exhibited, showing the whole disc of the Sun, on which groups of flocculi, generally lying in intermittent groups across the disc, appeared, and the surrounding prominences. Attention was called to some special cases of rapid changes in the prominences, and also to the fact that some prominences near the poles were more long-enduring than those at the equator. It was also mentioned that though it was rather expected that large prominences on the approaching limb of the Sun should be accompanied by large groups of flocculi near the limb, this was not always the case. The peculiar appearance of the structure of the flocculi was noted and reference made to the positions of spots in relation to the mass of flocculus in which spots are formed. A point strongly dwelt on was the relative short life of a spot in comparison with that of the flocculus in which it exists. Dr. Lockyer concluded by expressing his thanks to Sir Norman Lockyer for permission to read the paper, and to those who had assisted in this work.

The President. I think we can congratulate Dr. Lockyer not only on having got a perfect instrument, but also on the admirable work he has done with it, and we have to thank him for the pleasure he has given to us to-night. Another source of congratulation is the fact that although the Observatory with which he is connected is situated in London, it has not prevented him making very good use of his powerful instrument. The paper naturally divides itself into two parts — a description of the instrument itself, and results obtained. I hope that in the discussion we shall deal with both portions, and that one will not absorb our attention to the neglect of the other. I believe Mr. Newall has had something to do with the new spectroheliograph which has been made for the Madras Government. Perhaps he will tell us the differences between the instrument and Dr. Lockyer's. I will ask him to open the discussion.

Mr. Newall. The instruments are apparently both alike in this respect, that they are both at work and both seem to be giving admirable results. I may say that a few weeks ago I received from Prof. Michie Smith some photographs of his early successes, taken in November last year, and they seem very promising indeed, but I must not let this cause me to overlook my congratulations to Dr. Lockyer on his results. They seem to be admirable and full of all sorts of detail which will occupy some time in discussion, I imagine. With respect to the similarity of the instruments, I believe the image forming lenses are of the same make and of the same size. The dispersion in the Indian instrument is considerably greater than that used at South Kensington, and the comparison of the results got from the two instruments will for this reason be very interesting. I should like to ask Dr. Lockyer whether he has already got any information that he can give us about the relative durations of prominences and of the flocculi. I take it that the prominences on his photographs are all photographed in the K line, so they are calcium, which may be compared with the calcium flocculi. Has he compared the duration of the life of a prominence seen on the limb and of the flocculi at the centre of the disc?

The President. Do you know whether they have had any difficulty at Madras caused by the alteration of focus?

Mr. Newall. I do not know of any difficulty they may have had in that way.

Prof. Turner. I should like to contribute one statement to the discussion. I had the honour of representing this Society in the States last year—in September—at the Solar Physics Conference. Dr. Lockyer entrusted me with one or two samples of the beautiful work we have seen, and I showed them to Prof. Hale, who, I am glad to be able to report, admired the work very much and considered it was limited only by the size of the instrument. It is a small instrument, but, making full allowance for that, he considered the work was fully satisfactory in every way. I would, with diffidence, like to call attention to one point on the slide last shown—if I might have it on the screen again. It is a suggestion which seems worth making. It struck me that these two prominences suggested something of the nature of an obscuring screen. Their shape looks as though they might really extend down to the limb of the Sun, but that something between them and us gave them the peculiar shapes that we see. If there is any obscuration of that kind it might explain the apparent great velocities of prominences that have been observed, because a slight shifting of the matter upwards might cover them up, while a change downwards might reveal more large prominences. I throw that out as a slight suggestion, but I have been thinking about obscuration of the heavenly bodies generally, and that seems to me to be rather a striking possibility.

Mr. Maunder. With regard to the point Prof. Turner has raised, I think Mr. Newbegin made a recent observation of a prominence with a black hole right through it, and I might remind the Society that in 1900 some of the photographs that were got of the corona that year seemed to show definite dark markings—darker than the surrounding sky—as if there was some obscuring body between us and the corona.

Father Cortie. I must congratulate Dr. Lockyer most heartily on the first-class results which he has obtained with his spectroheliograph. It is gratifying, too, to see that he is paying attention to the relation of the flocculous masses to the different types of sun-spots. The correlation of the various phenomena in solar work is most important. With regard to Professor Turner's suggestion, dark solar prominences were observed by M. Trouvelot several years ago.

Mr. Newbegin. Mr. President, I take a great interest in this work, for, as you know, I have for many years been making prominence observations with the eye. With regard to the beautiful slides we have had to-night, I think one might almost put it in this way—one is lost in amazement at the beauty of the work, and also with the beauty of the instrument. I have been following Dr. Lockyer's remarks with the fullest interest all the way through, and I think there is promise of most excellent results. With reference to Prof. Turner's remark about an obscuring medium, I believe there is in all probability a great deal in it and we must all keep our eyes in that direction. As to the small prominence—only about 18″ in height—that Mr. Maunder mentioned, I may say that it quite astounded me. It had a black centre, almost as dark as ink. I had to pause and look at it for two or three intervals to see whether it was really so black.

Sir Robert Ball. I would like to add my congratulations to those of the Society in having such a paper—one of such very great interest—from Dr. Lockyer. We only see these phenomena in two dimensions, but there is a third dimension as well, and if we could see them in this, it might throw some light on aspects of them which we do not yet understand.

Dr. Rambaut. I want to ask a question or two, but before doing so I should like to add my congratulations to those which Dr. Lockyer has already received. The questions I wish to ask are in regard to matters of detail, not that I do not appreciate the beauty of this work, but I do not feel quite so much at home with these phenomena as some people appear to be, who can talk quite confidently with regard to operations taking place in the Sun. I would ask Dr. Lockyer first with regard to the clockwork movement of the slide. It seems to me to be a pity that some of these beautiful photographs should be spoiled through a defect in the movement, and I have been wondering why the oil form of control is used instead of something more dependable. Would not a mechanical governor, which would not be liable to break down, be better? The other question I wish to ask is this: the trouble to which he referred with regard to variation of the focal length—is not that a common feature in all these triple achromatics? I seem to have read the statement somewhere that the focal length of all Cooke lenses of this pattern do vary in a marked degree.

Mr. Conrady. I should also like to ask a question on this last point: Has Dr. Lockyer tried the effect of exposing the instrument to the rays of the Sun for several hours before making an exposure; it seems to me that a stationary condition must thus be reached and that the focus would then be fixed.

Mr. Franklin-Adams. This alteration of focus is sometimes a very serious thing. In a small transit instrument of mine of about 40 inches focal length the variation in ten minutes is often so much that the star-images become quite out of focus. It has often occurred to me whether it would be possible to have a table of focal length corresponding to temperatures and a little focussing-screw with graduated head to regulate it. It is a large question and perhaps opticians will take note of it.

Mr. Hinks. With regard to Mr. Franklin-Adams' remark about using a table of focal length as a function of the temperature, I may say that I tried for a long time to make an effective table for use with the Cambridge equatorial, but it looks in our case as if the focal length is a result of what has been happening to the object-glass in the last hour or two rather than a function of the thermometer-reading at the time.

Mr. Newall. I may say that arrangement is made on the Indian instrument for making the focal adjustment by visual observation before each photograph. I only hope that all parts of the lens will perform in the same way, but am afraid there will be zonal effects.

Dr. Lockyer. With regard to Mr. Newall's question as to the relative duration of the prominences and flocculi on the limb, when the photographs of prominences on the limb near the equator were secured, as a rule it was found that the next day they were not there; but, again, if the prominence happened to be near the poles of the Sun, then I think we have photographs in which we record the same prominences on two consecutive days. The flocculi on the disc last much longer than the spots. They are there a very long time before the spots are formed and remain there a long time after they have disappeared. We know that spots make several revolutions, so presumably in the case of the flocculi they must rotate with the Sun a great many times. This will be interesting to Mr. Maunder in relation to his fixed regions of the Sun to which he recently made reference. The fact is that this work seems to show that spots have very short lives and are only small indications of very large and extensive disturbances. Prof. Turner remarked the possible presence of some absorbing medium in relation to the forms of prominences: this point is one of great interest. These photographs might help considerably to continuously record the life of a prominence, say, during the whole day. I am afraid that Dr. Rambaut rather misunderstood me about the cause of the deformation of the disc-pictures. The clepsydra which controls the motion of the instrument from east to west works beautifully, the flow of oil being perfectly regular. The movement which I mentioned that produced the distorted discs was that of the siderostat. If that moves irregularly, then the image moves in relation to the primary slit and the result is some deformation of the form of the disc. The clepsydra works most satisfactorily, and I do not think one can detect any irregularity due to this motion. As regards the variation in the focal length of the Taylor 12-inch lens, I do not think it has anything to do with the temperature of the room. The change of focal length is very considerable in a short space of time. If, for instance, we expose a plate for a quarter of an hour and examine the focus afterwards we might be a quarter of an inch out! Everything seems to depend upon the solar beam falling on the lens. If a cloud passes across the Sun and obliterates the beam from the lens, then a change of focal length takes place. In the spring and the autumn we have to give comparatively long exposures, and such a change of focal length gives serious trouble. I believe that Sir David Gill has discarded most of the Taylor lenses at the Cape in consequence of this variation of focus. With regard to the last speaker's remark about the method of measuring the prominences, we simply measure the angle subtended at the centre of the disc of the extremities of each prominence, thus securing the full length of the prominence in latitude.

Mr. Maunder then read a paper on the large sun-spot of 1905, January 29th to February 11th, and the contemporaneous magnetic disturbances observed at the Royal Observatory, Greenwich. He said: The great sun-spot that we had at the end of January and the beginning of February made its first appearance, so far as we can tell, about the first day of the year. The first day we photographed it was on January 7th. During its first appearance it was quite a small group. (A photograph of the Sun taken on January 7 was thrown on the screen.) The principal spot was a small regular spot and there were a number of small faint spots following—too small and faint to come out well on the slide.

There was therefore nothing unusual in the appearance of the group early in January, but when it returned on 28th January it was a very large group indeed. On February 3rd the group was nearly in the centre of the disc, and from the preliminary measures it would appear to be the largest group we have ever photographed—it was about $\frac{1}{300}$th of the Sun's hemisphere in area. The Astronomer Royal has presented to the Society six original negatives of the Sun taken during the progress of this great group, and the slides now shown on the screen are reproduced from some of them. (A series of photographs of the great sun-spot were shown.)

During the period covered by these photographs, from January 1 up to the present date, there have been several magnetic disturbances. There was a very considerable one on January 5, and 27 days 4 hours later, one complete synodic rotation-period of the Sun, there was a marked disturbance on February 1. That disturbance did not last very long, but on February 3, at about $1^h\ 30^m$ A.M., a sharp disturbance began, the most distinct that we have had this year. The amplitude of movement in declination amounted to 45′ at 10 o'clock on the evening of February 3. Then, again, a complete rotation-period later there was another disturbance, from March 1st to March 2nd. (Reproductions of the photographic registers of the magnetic disturbances were shown on the screen.)

The President. We have another paper dealing with the spot, and I think it will be well to have the two discussed together. Mr. Fowler will deal with the spectroscopic aspect of the spot.

Prof. Fowler. The great Sun-spot which has been so well described by Mr. Maunder was not less remarkable when examined with the spectroscope. The weather was not very favourable, but I was able to make observations on four days during each rotation, and I was also fortunate in being able to observe the prominence phenomena when the spot passed over the western limb on February 11.

Besides the appearances presented by spots in general, the spectrum of the great spot exhibited reversals of lines which varied from day to day. C and F were constantly reversed until March 6, and the D lines were also seen bright over the spot during the first rotation. The most remarkable effects were witnessed between 9.45 A.M. and 10 A.M. on February 2. Outside the principal umbra the bright C and F lines were then displaced about 5 tenth-metres towards the red, though occupying their normal positions over the umbra itself; D_3 was a very prominent feature, as a bright line over the umbra, and changing from a bright to a dark cloudy appearance away from the umbra. Still more remarkable was the spectrum on the preceding edge of the principal umbra, where all the Fraunhofer lines were either obliterated or reversed, the bright lines actually identified being among those most frequently seen in the spectra of metallic prominences on the Sun's limb. All the bright-line phenomena are probably to be explained by the presence over the spot of prominences of varying activity.

The lines widened or strengthened in the spot appear to have been identical with those recently observed in other spots, and minute examinations of restricted regions of the spectrum from day to day indicated that, apart from the superficial effects already described, the spectrum was sensibly constant.

The spot-bands in the red, and those in the regions more refrangible than *b*, were strongly in evidence whenever looked for. Not only this; the general band of absorption was seen to present the appearance described by Young and Dunér; that is to say, it was not a continuous dark shading, as it appears with small dispersion, but was resolved into closely-crowded dark lines with bright gaps here and there, recalling the appearance of such a banded spectrum as that of sulphur, in which the maxima are not very pronounced. This resolution was best exhibited by the bright intervals, which were of the same brightness as the undimmed photospheric spectrum outside the spot. The appearances observed were in complete accordance with Young's view that the darkness of a spot is due to absorption by matter in the gaseous state, and not to defective radiation from the photospheric layer beneath the spot, or to absorption by solid or liquid particles.

The prominences overlying the spot on February 11, when it was on the limb, were very brilliant, and of rapidly changing form and size, and it may be remarked that in the delineation of such prominences visual observations would seem to have an advantage over the spectroheliograph. The chromospheric spectrum at the base of the prominences was a mass of bright lines, of which 67 were identified; the wave-lengths are given in the paper, and from a comparison with Young's observations it results that the spectrum of such disturbed regions is remarkably constant. Comparison with eclipse photographs further suggests that over a spot there is a general elevation of the chromospheric layers without much intermingling.

All the observations were made with a two-prism Evershed spectroscope attached to a 6-inch refractor, and I am glad to have the opportunity of stating that I have found this equipment very convenient and highly efficient.

Father Cortie. It was my privilege to observe with Professor Fowler's spectroscope this morning, and it is an excellent instrument, with, if I may be permitted to say so, an excellent observer at the end of it. The results of his spectroscopic examination of the great sun-spot are most valuable and suggestive. In particular it is a great feat to have observed more than sixty bright lines in the spectrum of the sun-spot in a small length of the spectrum, when the spot was on the limb, and to have identified these lines with those which Professor Young mapped in the chromosphere during his expedition to Sherman in 1872, and with the lines of the "flash spectrum" of total solar eclipses. This is a most important observation. The opinion expressed by Professor Fowler as to the substantial permanency of the spot-spectrum has been my opinion now for several years. I have some slides to show of the drawings of the big sun-spot as observed at Stonyhurst, of the magnetic storms, and of the photographed spectrum of the sun-spot. [Screen.] I would call attention to the positions of the bright bridge in the successive drawings of the spot, indicating a rotation of the sun-spot in a clock-wise direction as looked at on the drawings. A like phenomenon is depicted in the series of drawings of the October spot of 1900 made by Mr. Scriven Bolton, and published in the 'Memoirs of the British Astronomical Association' (vol. xii. part ii.). The direction of rotation is the same in both instances. This is a subject of investigation that would be an easy one for amateurs who draw sun-spots. With regard to the magnetic storms, I find that both began with sharp and sudden movements in the H.F. and declination magnets, at the dates 1905, February $3^{d}\cdot07$ and March $2^{d}\cdot56$, giving an interval 27·49 days. Using Spörer's formula, the synodic rotation-period of the spot in latitude −16° would be 27·71 days, differing by only 5·28 hours from the magnetic storm interval. This gives Mr. Maunder another sequence. The second storm was accompanied by a display of Aurora Borealis seen at Stonyhurst, March 2nd, 8 to 9 P.M., in which the dark segment and the overlying bright arch were well marked, with one fine streamer. The photographs of the spectra of the sun-spot include strips with λ 5590, the *b* lines, and H and K central. The H and K lines are reversed and many lines are widened. The photographs were taken on February 7 with the large spectrometer of the observatory. Unfortunately the means of feeding the instrument with light are inadequate, the image at the primary focus being only $\frac{5}{8}$ inch in diameter. Much better results could be obtained had we larger mirrors to fill the fine grating with light and give a larger image of the Sun.

Mr. Evershed. I am very much interested to learn that Prof. Fowler has been able to observe the banded structure in spot-spectra with one of my instruments. I understand, however, that Prof. Young, in describing this feature, states that he counted some hundreds of bands within the "*b*" group, and it seems almost incredible that Prof. Fowler's two-prism spectroscope should have sufficient resolving power to show the bands. I have myself often observed spot-spectra with a more powerful spectroscope than that used by Prof. Fowler, but I have never succeeded in seeing the banded structure *. I would like to ask Prof. Fowler what particular region of the spectrum the bands appeared most distinctly?

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* [It has sometimes happened that an exceedingly fine banded structure has appeared both in the spot-spectrum and on the general surface of the photosphere. This, however is a spurious effect, and is due to diffraction-bands caused possibly by the jaws of the slit not being absolutely in one plane. It is curious that these bands appear to be more easily seen in the green than elsewhere in the spectrum, at any rate in my own instrument.—J. E.]

Mr. Hinks. I am wondering whether Mr. Fowler's comparison of the times taken to observe visually and photographically is quite fair. So far as I can understand, he can make a drawing of one prominence in about a minute, and Dr. Lockyer can photograph all the prominences round the Sun in 10 minutes; and as the number of prominences is a variable quantity, I wish to question whether it is a fair comparison.

Dr. Lockyer. At present there is no difficulty in pursuing any one prominence only, whatever part of the Sun it is in.

Mr. Newall. I was much interested in Mr. Fowler's observations of the D_3 line. It is a very unusual case to see the D_3 line on the Sun *dark*. I have only seen it recorded once before.

Mr. Evershed. I have seen it once only in 10 years.

Mr. Newall. I have seen such an observation recorded once in the *Astrophysical Journal* during the last few months, but Mr. Fowler has not only seen this line turning black, but has also made observations of the reversal of the line beside it.

Mr. Shackleton. I can confirm Mr. Fowler's observations. The same day, in the afternoon, I saw D_3 as a dark cloud.

Prof. Fowler. Notwithstanding Mr. Evershed's remarks, I am quite convinced that I have seen the appearances of the spot-band as described by Young and Dunér. In the region more refrangible than *b*, and from *b* to about λ 550, the bright intervals were comparatively easy of observation, and others to whom I pointed them out had no difficulty in seeing them. I had not recognized the resolution previously, and I imagine that in the great spot the structure of the band was better seen for the same reason that the lines in stellar spectra are more visible when the spectrum is broadened by a cylindrical lens.

In connection with what Mr. Hinks has said, I admit that the comparison was not quite fair in the way I put it. What I meant to say was that if it takes five minutes to produce a photograph, the resulting form will not correspond with the actual form at any instant in the case of such rapidly-changing prominences as those of February 11.

Father Cortie. The line D_3 has also been seen as a dark line in sun-spots at Stonyhurst. As to the question of the breaking up of the dark general absorption-band of the sun-spot spectrum into spaces separated by bright intervals, Prof. Fowler showed me the phenomenon this morning in the spectrum of a small spot. I am not quite sure that what I observed was not a contrast effect, and quite subjective. The appearance would be more convincing if seen in a spot with a broad band of general absorption. This question is a very important one, for if the black band in a spot-spectrum is made up of a series of lines and bands separated by bright bands, it would show that the blackness is due to absorption by gases, and not, as is now generally held, to the stopping and scattering of light by solid and liquid particles amid great thicknesses of metallic vapours.

The President. Mr. Maunder has got a further paper on the association of magnetic disturbances with sun-spots, and if he can give a summary of it in the seven or eight minutes we have left, we shall be very pleased to take it.

Mr. Maunder. I trust that the Society will not think that I am blossoming out into a "hardy monthly" on the subject of magnetic disturbances, but there were one or two points on which I wish to say a few words with regard to some criticisms on my paper of last November. There are three points I want to refer to:—First, the general character and scope of the paper; second, the demonstration of what I will call the "interval relation"; and third, the question of the distribution of centres of activity upon the Sun. First of all, with regard to the general scope and character of the paper: Prof. Schuster, in his comments on my paper, seems to have quite misunderstood that character. I cannot take to myself any blame for his misunderstanding, because, so far as I have been able to see, no one else seems to have shared it; but it is an important one. He regarded my paper as a theoretical one—as an attempt to get over Lord Kelvin's celebrated difficulty by means of a theoretical suggestion. My paper was entirely observational; I brought forward no theories whatsoever, nor did I in the slightest degree discuss the abstract question as to where the energy of magnetic storms came from. The next point was the demonstration of what I will call the "interval relation"—that is to say, the tendency of magnetic disturbances to recur at intervals approximately equal to the synodic rotation of the Sun. That question is one with which it is a little difficult to deal strictly, because of the number of periods and number of sequences which are involved; but if we take a sequence at a time separately from the rest, the case is quite simple. Thus, in my sequence XXV., if we take the first and last members, and regard the question as one of the probability of the two intervening disturbances having come exactly at equal intervals between the first and the last, it is entirely simple. The deviations from absolutely equal intervals are less than 1°·6; doubling that, so as to allow an equal range on the other side, we get 3°·2 as the range we have to allow. Then dividing the whole distance from first to last into 338 sections, each section will more than cover the entire double range. We may regard the question thus: that out of the 338 sections, 334 are undisturbed and 4 are disturbed, of which 2 are in sequence. Sequence LIII. is a longer one, of seven members. It is a combination of two sequences, that I gave in my November paper, completed by the insertion of a new disturbance. This disturbance was quite as large as some of the others, but was not given in the plates of the Greenwich volume, only in the "Notes." Treating this sequence in the same way, we find the probability of the sequence being an accidental occurrence is only about 1 in 5 millions.

I was very gratified to find that there was a general consensus of opinion that the "interval relation" had been proved. Prof. Schuster definitely stated his opinion to that effect; and Father Cortie, though he took an antagonistic view on the whole, admitted that many of the sequences were real; whilst Prof. Larmor said that my paper "amply satisfied him that these magnetic storms somehow or other occur in a period which is nearly the same as that of the rotation of the Sun." Prof. Schuster, however, decided to examine my catalogue of disturbances by means of his periodogram; and the result of that examination has been supposed by some to be only favourable in a restricted degree to the reality of the "internal relation." But the problem before us is not a suitable one to be so tested by the periodogram. Its unsuitability does not result from the fact that no continuous action is concerned, and hence no true periodicity, but from this fact combined with two others—viz., that an indefinite number of periods are concerned differing from each other through wide limits, and that no one of these periods is in operation for more than a relatively limited time. Curiously enough, Prof. Schuster has himself pointed out these circumstances incidentally in one part or another of his paper. Instead of testing only four selected periods, he should have tested seventy at least, and one hundred would have been better.

But one detail came out in the course of Prof. Schuster's analysis. It was one to which I had already drawn attention in my paper, and it is suggested by the appearance of the diagram which I had prepared—viz., that there is a tendency in the disturbances to concentrate along two particular longitudes. Prof. Larmor suggested that if I tried some other rotation-period than Carrington's, it might bring out other striking relations, for many of the disturbances appear to form sets running in a slanting direction across my diagram; and he made the further suggestion that it would be very much better to take sun-spots and treat them in the same way. Mrs. Maunder and myself have been engaged upon that for some little time; and, just as an illustration of what we are doing, I have prepared another diagram. We have taken all the spots during the cycle 1889–1901 in the Northern Hemisphere from the equator to 7½° N. latitude, and plotted them down, not with the Carrington period, but a period of one-third of a day short of it. We could have improved upon that period just a little, but I think the relation which it brings out is sufficiently striking as it stands. Here we have, for the whole of the 10 years which the diagram covers, great spaces in which there are no important spots at all. The cases of condensation are very distinct. I should say that necessarily the small spots are very much exaggerated in size, and that the very large spots are represented by less than their proportionate area. In the lower part of the diagram we have a period of three complete years, in which practically all the spots are confined to 140° of longitude, whilst in the remaining 220° there is practically nothing.

The following papers were announced and partly read :—

E. W. Brown. Theory of the Motion of the Moon.—Part IV.

Max Wolf. The Nebula near ψ Eridani.

C. J. Merfield. Observations of Uranus and Saturn.

John Tebbutt. Observations of Uranus at Windsor, New South Wales.

W. J. S. Lockyer. The Spectroheliograph of the Solar Physics Observatory.

Rev. S. J. Johnson. The late Leonids of November 1904.

E. W. Maunder. Magnetic Disturbances and their Association with Sun-spots.

On the large Sun-spot of 1905, January 29 to February 11, and the Contemporaneous Magnetic Disturbances, observed at the Royal Observatory, Greenwich. (Communicated by the *Astronomer Royal.*)

H. C. Plummer. Notes on the Siderostat and Coelostat.

A. C. D. Crommelin. Provisional Elements of Jupiter's Satellite VI.

William Ellis. The Seasonal Variation in Magnetic Disturbance; with other Remarks.

A. E. Conrady.—The Optical Sine-condition.

W. W. Bryant. Note on the Publication of Astronomical Papers; with special reference to the International Catalogue.

A. Fowler. Spectroscopic Observations of the recent Great Sun-spot and Associated Prominences.

The following gentlemen were elected Fellows of the Society :—

Brandon T. Brierley, F.G.S., Assoc.M.Inst.C.E., Linthwaite, Delph, Yorks.

Maurice Farman, Observatoire de Chevreuse, à Jagny, par Dampierre, Seine-et-Oise, France.

Willie Venner Merrifield, B.A., Nautical College, Byrom Street, Liverpool; and 5 Green Bank, Waterloo, near Liverpool.

Isaac Molloy, M.A., Lützen, Glenageary, Kingstown, Dublin.

Alfred Edward Nicholls, King Edward VII. Nautical School, London, E.; and Cotswold, Hornchurch, Essex.

John Wearing, Garsdale, Sedbergh, Yorks.

The following Candidates were proposed for election as Fellows of the Society :—

Scriven Bolton, 24 Kensington Terrace, Hyde Park, Leeds (proposed by Richard Kerr).

Balme Bonniksen, 16 Norfolk Street, Coventry (proposed by Richard Inwards).

Edward Turner Cottingham, Scientific Instrument Maker, The Limes, Thrapston (proposed by Julien Tripplin).

William George Hooper, Nottingham (proposed by Richard Kerr).

Percy Marshall, Librarian, Filey House, Livingstone Road, Scarborough, Yorkshire (proposed by the Rev. T. E. R. Phillips).

Karl Pearson, M.A., LL.B., F.R.S., Professor of Applied Mathematics and Mechanics, University College, London; 7 Well Road, Hampstead, N.W. (proposed by H. H. Turner).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 February 22.

President: A. C. D. CROMMELIN, in the Chair

Secretaries: J. A. HARDCASTLE.
J. G. PETRIE.

Mr. Petrie read the Minutes of the previous Meeting, which were confirmed.

Mr. Hardcastle read the list of presents received, amongst which were Comte de Miremont's 'Popular Star-Atlas' and Mr. S. R. King's MS. Reduction of the Positions of Messier's Nebulæ for 1900.

The names of two new Members and three Candidates for Membership were read.

Mr. A. Stanley Williams contributed a paper on "Light Changes on V Vulpeculæ." Observations in 1903 seemed to show that this star was of the β Lyræ type, with a period even longer than R Sagittæ, namely 75·3 days. Last year, more conclusive results were obtained, and the exact form of the light-curve well ascertained.

The magnitude at the two maxima was 8·31, at the principal minimum 9·75, and at the secondary minimum 8·66. The dates of the principal minima furnished by the observations of 1904 were, June 6, August 22·5, and November 6. The observed date of the secondary minimum was September 29. A possible variation in colour was suspected.

Mr. E. W. Maunder showed a series of slides of the recent great sun-spot. He remarked that they had just had the opportunity of seeing on the Sun the largest sun-spot of the last 33 years. It was first seen on January 28th, crossed the central meridian on February 4th, and passed off on February 11th. Expressed in English miles, the total length of the group was rather over 100,000 miles, and the breadth, in its widest part, was about 60,000 miles. The total area was not very far short of 4000-millionths of the Sun's visible hemisphere—that was to say, about 1-270th of the Sun's hemisphere. It was therefore slightly larger than the great spot of February 1892, which until then was the largest upon the Greenwich record. The spectroscopic examination of the group was of extreme interest, the whole region of the Sun in the neighbourhood of the spot being most disturbed. A distinct, but not exceptional, magnetic disturbance occurred just about the time when the first edge of the group reached the central meridian. Regarded as a magnetic disturbance, however, it was not at all remarkable.

Mr. C. Thwaites also showed some slides of the great sun-spot.

Mr. Maunder, referring to a photograph of February 8th, said he was very much struck by the beautiful way in which the Wilsonian effect seemed to be reversed in a great group as it was there seen. The penumbra was broadest on the inside instead of on the outside, as one would expect from the Wilsonian theory.

Mr. Thwaites said that was so, but he had seen, on two or three occasions lately, spots which bore out the Wilsonian theory.

Mr. Maunder added that he had occasionally seen a pair of spots go off the limb together, one appearing as this one of the 8th did, and the other showing the Wilsonian effect quite distinctly.

The President. But was that umbra at an earlier stage central in the spot or on the side next the limb?

Mr. Thwaites thought nearly central, but the spot changed greatly.

Mr. G. F. Chambers pointed out how much might be done in this class of work by the possessors of small telescopes. Even a 2-inch telescope would show a great many things which were worth looking at.

Colonel Markwick said he had a very good view of this great spot with a 2¾-inch telescope. The amount of detail that was visible through this telescope, with a magnifying-power of about 70, was something wonderful.

Mr. Alexander Hutt asked if it were possible to say on what day the spot was first forming.

Mr. Maunder said this was the second appearance of the group. It came on first just about the beginning of the year, 1st or 2nd January. It had not returned again yet; it should be back by Saturday, February 25.

Mr. Maw said that on February 4 the spot was quite a prominent object to the naked eye, being easily seen with a light-shade. In the evening he saw it through a slight haze, without any shade.

Mr. Thwaites remarked that the spot was visible to him every day with the use of a shade, and he had seen it, as Mr. Maw had done, through thin cloud.

The President said that it was a little disappointing that the magnetic and auroral displays were not more striking. It was by no means always the biggest spots that offered the most in that way. He agreed with Mr. Chambers that the Sun seemed *par excellence* the object for possessors of small telescopes to work at. He believed that Schwabe used quite a moderate instrument, 3-inch or so, with which he discovered the periodicity of the spots.

Mr. G. J. Newbegin read a paper on "Solar Prominences in 1904." He remarked that whereas in 1903 52 observations yielded 197 prominences on 41 days, the remainder being blanks, 1904 had yielded 174 prominences on 24 days, there being no blanks, thus giving the year now closed a larger proportion of prominences for the number of observations. Unfortunately, work had to be suspended at an early date owing to removal.

Mr. Maunder said that the last observation Mr. Newbegin had drawn attention to was a very interesting one. He had never seen anything of the kind himself, but some years ago M. Trouvelot, who was at that time a regular prominence observer, reported on more than one occasion having seen what he called a "black" prominence—that was to say, a distinct black shape on the background of a bright prominence. With regard to the general course of sun-spot activity during 1904, it had been an uneventful year. In 1903 the curve seemed to be running up very fast. 1904 had been one of very steady increase, but showing nothing like the rapidity of increase that took place in the corresponding period of the two previous cycles. There were no days during the whole of 1904 without spots.

Mr. Lynn. Do you think maximum is past?

Mr. Maunder. I do not think it is. After what we have seen the last fortnight, one may look out for anything.

The President said there were very few people in England who carried on this solar prominence work, and they were all very glad that Mr. Newbegin was carrying it on so regularly, and he had been rewarded last year by seeing several notable outbursts.

Mr. Hardcastle then read a note on "The Partial Lunar Eclipse of 1905, February 19," by *Mr. W. C. Tetley.*

Messrs. Thwaites, Newbegin, C. L. Brook, and *Col. Markwick* gave their impressions of the eclipse, and *Mr. Saunder* read a letter from *Prof. Moye*, of Montpellier, France, on the same subject.

The President said there seemed a general consensus of opinion that the eclipse was an unusually bright one. He always thought it the chief sentimental interest of a lunar eclipse that we looked at our own world, as it were, from the outside, and saw the actual round outline of our own globe.

Mr. Gavin J. Burns read a paper on "An Instrument for Measuring the Brightness of the Sky," and also exhibited the instrument. The instrument consisted of an ordinary finder having the object-glass fitted with an iris diaphragm. In order to determine the brightness of the field of view, the diaphragm was gradually closed until the cross wires disappeared. The brightness was then inversely proportional to the square of the aperture.

In order to compare observations made on different evenings, it was necessary to obtain a measure of the clearness of the atmosphere. This could be done by making a preliminary observation of some faint star near the Pole. The aperture at which it ceased to be visible was noted, and the transparency of the atmosphere deduced from the observation.

The President said the device explained by Mr. Burns was a beautifully simple one, and would prove very effective in his hands, but he ought to make some observations in a place some miles away from sources of artificial light. There could be no doubt that the results attained would be of greater weight if that could be done.

A discussion followed on the arrangements in progress for seeing the coming total solar eclipse. *Mr. T. R. Clougher* mentioned that the Dominion Government were arranging for a party to go out to the coast of Labrador, and had invited six members of the Royal Astronomical Society of Toronto to join that party.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

At the Meeting of this Society held on the evening of Wednesday, March 15, at the Institution of Civil Engineers, Great George Street, Westminster, the President, *Mr. Richard Bentley*, delivered an address on "The Growth of Instrumental Meteorology."

After briefly touching on the historic and non-instrumental era of Meteorology, he spoke of the seven great weapons of meteorology —the thermometer, and of later years the heliograph, for temperature; the hygrometer and rain-gauge for moisture; the barometer for pressure; and the anemometer and kite for the study of the upper air; and of the great foundation of instrumental meteorology laid by Galileo, Torricelli, Wren, and Hooke. The President, in dwelling upon our indebtedness to Italy for Science (as well as for Art) from Galileo to Marconi, pointed out that the theory of rainfall was correctly enunciated as early as the beginning of the fourteenth century by Dante. He also dwelt on the great services rendered to the community by meteorologists, largely by volunteers at their own expense, and referred to the close observation kept by rain-gauges on the steadily diminishing water-supply of the country, by anemometers protecting the traffic over some of our lofty and more exposed railway viaducts, by the use of the barometer for storm-warnings and for the safety of miners in our pits, by the heliograph with relation to the ripening of fruits and crops, and regretted how much of the immense mass of information daily accumulating had still to be analyzed and put to use. It was disappointing to find in so wealthy a country as this, and where the results could not fail to be of the greatest practical utility to the nation, that the means of digestion of this vast data are so meagre, and the aid given by the Government is so slender, as to be a constant source of reproach when compared with the large provision made for the same purpose in other countries for their own benefit.

Mr. W. Marriott also exhibited a number of lantern-slides illustrating meteorological phenomena.

At the close of the Meeting the Fellows and their friends inspected the Exhibition of Meteorological Instruments which was arranged in the Library of the Institution of Civil Engineers.

Annular Eclipses.

Early this morning an eclipse took place in southern regions of the type termed annular, in fact one in which, owing to the differences of diameters of Sun and Moon, the ring would be seen to perfection on the central line. The central and annular phase visits the continent of Australia, passing off the latter somewhere in the neighbourhood of Rockhampton. It is to be hoped that some observations have been obtained. Some years have passed since any results worth mentioning have been obtained from annular eclipses.

On the occasion of that of June 6, 1872, Pogson, at Madras, noticed the solar spectrum reversed, the inference being that in the lower part of the solar atmosphere there exist vapours of metallic elements. But observations made in India during the total eclipse of 1898 indicated an opposite result, "the identity of the spectra at second and third contacts, that is, at the opposite limbs of the Sun. showing clearly that the photographs are spectra of the true chromosphere not due to any local conditions such as the presence of a metallic prominence" (see 'Monthly Notices,' February 1899).

Though mention is made of an annular eclipse in A.D. 334 in some of our astronomical works, yet no mention of the actual ring-form occurs, and probably not in any eclipse until the year 1263, August 5, when Haco, King of Norway, having anchored his fleet at Ronaldsvoe (Orkney Isles), the Sun in the middle of the day appeared as a "thin bright ring." Perhaps the next account of an annular eclipse is that of 1601 December 14 (there being a doubt whether that seen at Rome by Clavius in 1567 was total or annular for a few seconds). In 1601, we are told, "this eclipse was also observed by the fishermen upon the sea-shore near Bergen in Norway, where they, with great admiration, saw the whole body of the Moon to be contained within the Sun, so that there did appear a bright circle round the Moon" (Wing's 'Harmonicon Celeste,' p. 157). In Tycho Brahe's 'Historia Celestis' there is also a drawing of the annular phase as seen at Nidrosia (Drontheim), Norway.

But the curious point to which I would call attention is the comparative frequency of these phenomena in past centuries and their rarity in years to come. In the 16th century there were two annular eclipses crossing England, those of 1502 and 1547. As an old chronicler tells us that Henry Lord Clifford built Barden Tower, in Yorkshire, in the year 1502 for the pursuit of astronomy, "in which he did greatly delight," is it not a probable conjecture that the eclipse of that year induced him to carry out this work, just as that of 1560 set Tycho Brahe pondering on the heavens? In the following century we had two, those of 1601 and 1621. In the 18th century three crossed Great Britain, those of 1737, 1748, 1764, and, if we include the Shetlands, that of 1793 also. Those of 1737 and 1748 were well observed in Scotland, a trace of the corona and red flames being seen on each occasion. The following words of Colin Maclaurin at Edinburgh (Phil. Trans. vol. 40) may be noted:—"There was something very entertaining in the annular appearance, a phenomenon that was equally new to all who saw it, that gave great delight to the curious, without striking terror into the vulgar." Considering the abnormal interval of more than two centuries (1724–1927) without a total phenomenon across Britain, many of us would wish for this "terror."

Coming to the 19th century, four annular phenomena cross our islands, that of 1820 (Shetlands), those of 1836, 1847, 1858. That of 1820 was annular on the Continent for more than five minutes, but must have declined at each 18-year Chaldean period more rapidly than is common, for in the year 1874 the annular phase was confined to the neighbourhood of the river Obi in Siberia, and in 1892 it had ceased to be central at all. Passing over the 1836 eclipse as being noted for the irradiation phenomena called "Baily's Beads," it may be noted that the track in 1847 was very wide, Plymouth being nearly on the central line in England, and Rouen and Chalons in France. The northern limit included Limerick, reached almost to Gloucester, and the whole track extended in France from Orleans to a point north of Lille, but so cloudy was the weather that I cannot find any observation except at Orleans, just on the very limit of the annular track. The atmospheric conditions were not much better on March 15, 1858, as many of us remember. Still, the ring was seen for seven or eight seconds near Charmouth, for an instant or two in the market-place at Devizes, and for a few seconds at the railway-station at Peterborough. Mr. Breen, at Cambridge, off the central line, seems to have had a faint glimpse of the corona for a few seconds, the only instance of this being seen in England during the 19th century.

Contrast the foregoing with succeeding centuries. In this 20th century, the only time annularity is presented in Great Britain is in April 1921 across the Hebrides, a very small slice of the mainland of Sutherlandshire also being included. Nothing more occurs until July 2093, when an annular eclipse happens under very favourable circumstances across the more northern counties of England. Its successor does not come on until September 2220, when, according to Dr. Hind, the annular phase may include a portion of the west of Cornwall *. The reason why annular eclipses do not appear on record by our forefathers is, doubtless, because they did not distinguish them from large partial ones.

While on the subject of annular eclipses it may be mentioned that at a point in Portugal, probably north of Lisbon, there seems to have been a case of an annular eclipse visible in two successive years, about three-quarters of an hour before sunset on January 27, 1683, and again on July 12, 1684. It is rare for two *partial* eclipses close together to be visible at the same place, but this has occurred in Great Britain once in the last century, when half the Sun's diameter being obscured at Greenwich on October 9, 1866, there was again an eclipse of three-fourths on the 6th of the following March. In the previous century, there was the extraordinary case of three small solar eclipses being visible at Greenwich in succession, those of April 20 and October 17, 1781, and that of 1782 April 12. This circumstance must be almost unique.

In 1908 there will be another unusual occurrence, three solar eclipses and no lunar one, a case which last happened in 1535.

A central eclipse of the Sun at any particular place is a phenomenon of rare occurrence. It is therefore worthy of notice that at a spot near the western extremity of Africa, in lat. about 9° north, and long. about 7° west from Greenwich, in the year 1835, both the annular eclipse of May 27 and the total one of November 20 would be central and visible, and there would also be visible at the same spot the whole of the eclipse of the Moon, and the beginning of the transit of Mercury, that year! And four planets were very near the Sun during the total eclipse. But the time for total-eclipse observations had not arrived.

Melplash Vicarage, Bridport,
1905, **Mar. 6.**

S. J. Johnson.

Magnetic Storms and the Solar Rotation.

Most readers of Mr. Maunder's paper (M. N. vol. lxv. p. 2) probably formed their opinion of the real or accidental character of the recurrences of magnetic storms at intervals of 26·7 to 28·7 days (the periods of rotation of sun-spots) by running their eye down the diagram or noticing the number of sequences given in Table III. (p. 19) as compared with the total number of storms collected in Table I. The diagram and tables display the result so well that it is quite possible to form a good opinion in this manner. It may, however, be of interest to consider the probability of the relationship in a little more detail.

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* Dr. Hind, a few months before his death, having requested me to search for annular eclipses in future years, I found nothing except that of A.D. 2093 until the year 2350 (when the conditions seem fulfilled), but informed him that the eclipse of September 26, 2220, might touch the S.W. corner of England in its annular phase. He replied as follows: "Oppolzer-to-Ginzel gives an annular eclipse in part of Cornwall on Sept. 26, 2220, the north limit passing nearly through Camelford and St. Germans. In case you like to leave a memorandum in the archives of Melplash for the benefit of any astronomically disposed vicar of that day, I can also tell you that the greatest eclipse will occur there at $1^h\ 5^m{\cdot}1$ local M.T.; magnitude 0·936."

The simplest analogous case which occurs to me where we know the cause of the recurrences may be found in the observations of stars made for clock-error. Thus, if A, B, C, D, &c. be the right ascensions of a number of different clock-stars, on one day the stars A, B, C might be observed, on another B, D, E, &c., and the clock-book for a few days might show something of this kind:—

Day.		Stars observed.									
1	……	A	B	C							
			|								
2	……		B		D						
3	……					E	F				
							|				
4	……	A					F				
5	……	Nothing observed.									
6	……				D			G		K	L
7	……								H		
									|		
8	……								H		

We have three cases here in which the same star was observed on consecutive days, or what Mr. Maunder calls *sequences* in his paper. Suppose now that we had simply given us the sidereal times at which the above stars were observed on the different days. The time of the observation of B on the second day would agree with that on the first day within, say, ¼ of a second, and the chance of this being a fortuitous coincidence is evidently extremely small (of the order of 1 second to 1 day). If there were 30 sequences of this kind in, say, 200 events, the possibility of their being fortuitous would instantly be seen to be absolutely infinitesimal.

Suppose, now, the times of occurrence of these 200 events with 30 sequences were analyzed harmonically. We should expect the coefficients for the period of one sidereal day and its harmonics to be 1·3 times as large on account of the sequences as they would be were the grouping entirely fortuitous. If only a few harmonics were taken the analysis might not suggest that there was any relation to the sidereal day in the numbers, but if sufficient were taken the excess of the coefficients over the normal expectancy would be perceived. Still, in this case, the method of the periodogram would give only a feeble indication of what, in sporting language, would be seen to be a "dead certainty" when the ordinary methods of probabilities were applied to the original figures.

The case presented by Mr. Maunder is very closely analogous to the above example from the clock-book, and the ordinary methods of probability are readily applicable. Instead of the sidereal day we have the mean value adopted for the solar rotation, and instead of the small range of a fraction of a second we had in the clock-book two magnetic storms may be taken as forming a sequence if they occur in consecutive rotations, and the longitudes of the central meridian of the Sun at the times of occurrence agree within 10°.

Taking Mr. Maunder's list of magnetic storms, it will be found that there are:—

(i) 49 pairs of consecutive rotations, in each of which one, and only one, storm occurs.
(ii) 39 pairs in which the numbers of magnetic storms are 2 and 1 or 1 and 2.
(iii) 20 pairs in which the number of storms are 2, 2; 1, 3; or 3, 1.
(iv) 11 pairs in which the number of storms are 2, 3; 3, 2; 4, 1; or 1, 4.
(v) Rotations Nos. 540 and 541 with 4 and 2 storms respectively, and Rotations Nos. 513 and 514 with 5 and 3 storms respectively.

Inserting the numbers of storms in each individual rotation given in Mr. Maunder's Table III., we find:—

(i) 18* sequences in the 49 pairs (1, 1).
(ii) 10 „ „ 39 „ (1, 2 or 2, 1).
(iii) 7 „ „ 20 „ (1, 3; 3, 1; or 2, 2).
(iv) 9 „ „ 11 „ (2, 3; 3, 2; 1, 4; 4, 1).
(v) 1 sequence in Rotations Nos. 541 and 542 (4, 1), and 2 sequences in Rotations Nos. 513 and 514 (5, 3).

In Class (i), which is the most important, the probability of a sequence, if the occurrence of storms were entirely fortuitous, would be $\frac{1}{18}$, so we should expect 2·7 in 49, and actually have 18. Similarly, in Class (ii) we should expect 4·3, and have 10; in Class (iii) we should expect just under 4, and have 7; in Class (iv) we should expect about 3, and have 9. The probabilities of the two cases occurring in Class (v) are $\frac{2}{9}$ and $\frac{65}{408}$ respectively. In each case, therefore, the number of sequences are well in excess of the number a fortuitous grouping would give.

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* Rotations 438 and 439 are counted as giving one sequence, though the difference is 10°·4.

The chance of obtaining 18 sequences in Class (i) as the result of accident is extremely small. The successive terms in the expansion of $(\frac{17}{18}+\frac{1}{18})^{49}$ give the respective probabilities of 0, 1, 2, 3, &c. sequences occurring by chance. The values of the successive terms are given in the following table:—

Number of recurrences.	Probability.	Number of recurrences.	Probability.
0	0·060	10	$2·48 \times 10^{-4}$
1	0·175	11	$5·17 \times 10^{-5}$
2	0·247	12	$9·62 \times 10^{-6}$
3	0·227	13	$1·61 \times 10^{-6}$
4	0·155	14	$2·44 \times 10^{-7}$
5	0·082	15	$3·34 \times 10^{-8}$
6	0·035	16	$4·18 \times 10^{-9}$
7	0·013	17	$4·78 \times 10^{-10}$
8	0·004	18	$4·99 \times 10^{-11}$
9	0·001		

Thus in 1000 cases we should expect 6 sequences 35 times, so that if there had only been this number they might well have arisen by chance. The chance of nine sequences occurring by chance is only 1·05 per 1000, and after this the probability diminishes extremely rapidly, so that the chance of 18 sequences occurring fortuitously is only 1 in 20,000 millions.

This number is so small that it is not necessary to consider the other classes in detail, though each would contribute a considerable factor towards still further diminishing the chance that the sequences in Mr. Maunder's Table III. are accidental. Further, as Mr. Maunder has pointed out, the chance of any one sequence extending over 4 or 5 rotations arising by accident is extremely small.

The recurrence of magnetic storms at intervals within the rotation-periods of sun-spots in different zones, announced by Mr. Maunder, has, it seems to me, a high degree of certainty, and the purpose of this note is to show how readily a formal calculation of the chances may be made, and how it confirms the opinion formed by inspection of Mr. Maunder's figures. F. W. D.

The Solar Eclipse of 1905 *in Spain.*

The total eclipse of 1851 in Sweden and Norway, the first ever photographed; of 1869 seen from the middle United States, when Prof. Young discovered the characteristic line in the spectrum of the corona; of 1887, whose track through Russia, Siberia, and Japan many observers still living remember to their discomfiture—the eclipse of this famous Saros series returns again the present year to regions which, for accessibility and chances of good weather, are especially favourable.

In the course of a thorough survey of the entire track of this eclipse, we have had occasion to calculate the times of the phases and other data for a wide choice of stations throughout the belt of totality.

This is the fifth total eclipse visible in Spain during a period of 63 years; and this very interesting country will doubtless draw the largest number of observing parties from England and the United States, although Algeria, Tunisie, and Egypt present many attractive conditions, in spite of the eclipsed Sun at lower altitudes.

In the subjoined table are given these calculated data for many places that may readily be identified on any good map of Spain. For completeness and ease of reference, we have added also the similar data as computed by Dr. Downing, and already published in the 'Eclipse Circular of the British Nautical Almanac':—

	Lat.	Long. (from Greenwich).	Eclipse begins.	Totality begins (Local Mean Time).	Totality ends (Local Mean Time).	Eclipse ends.
SPAIN.	° ′	° ′	h m s	h m s	h m s	h m s
Coruña	43 21 N.	8 25 W.	23 4 11·4	0 25 10·2	0 26 37·3	1 45 24·0
Ferrol	43 30	8 14	5 10·3	25 29·8	28 6·9	46 12·3
Lugo	43 2	7 33	9 14·5	30 14·3	32 0·9	50 32·2
*Luarca	43 29	6 34	14 39	34 22	38 7	55 11
Oveido	43 20	5 49	19 4·2	38 51·6	42 32·9	59 27·0
Leon	42 38 N.	5 34 W.	23 22 49·4	0 40 45·9	0 44 13·6	2 1 54·7
Valladolid...	41 39	4 41	26 34·7	48 46·9	49 21·7	8 5·1
Palencia ...	42 3	4 28	27 44·9	48 19·9	51 39·6	8 45·1
Burgos	42 20	3 43	30 34·1	50 44·1	54 27·6	11 5·4
Soria	41 44	2 31	39 22·5	59 39·7	1 3 23·0	19 42·5
*Ateca	41 17 N.	1 51 W.	23 43 38	1 4 4	1 7 49	2 24 1
Castellon ...	39 59	0 4	55 19·5	15 47·5	19 14·5	35 49·0
Torreblanca.	40 15	0 14 E.	57 14·2	17 48·4	21 30·9	37 8·4
Palma de Mallorca	39 37	2 43	0 11 44·1	32 17·8	35 25·8	50 27·3

* Near.

	Duration of Totality.	Magnitude of Eclipse.	Position-angle (direct image).				At middle of Eclipse.	
			First Contact.		Last Contact.		Sun's Azimuth.	Sun's Altitude.
			N. Pt.	Vertex.	N. Pt.	Vertex.		
SPAIN.	m s		°	°	°	°	° ′	° ′
Coruña ……	1 27·1	1·002	295	312	113	84	10 59	55 21
Ferrol ……	2 37·1	1·007	294	312	114	84	11 21	55 11
Lugo ………	1 46·6	1·003	294·8	311·1	113·4	82·6	13 19	55 27
*Luarca ……	3 44·7	1·020	294	308	114	84	11 26	55 11
Oveido ……	3 41·3	1·019	293·5	306·7	114·7	82·6	17 16	54 42
Leon ………	3 27·7	1·014	294·4	306·8	114·1	80·7	18 17	55 16
Valladolid…	0 34·8	1·000	296	307	114	78	21 36	55 47
Palencia …	3 19·7	1·013	294·8	305·9	114·1	78·7	22 11	55 16
Burgos ……	3 43·5	1·020	294	304	115	79	22 42	54 54
Soria ………	3 48·3	1·021	294·3	301·7	114·9	77·5	26 39	54 46
*Ateca ………	3 45·0	1·020	295	300	115	76	28 43	54 48
Castellon …	3 27·0	1·014	295·2	297·2	114·4	72·6	34 13	54 44
Torreblanca.	3 42·5	1·020	295	296	115	73	34 52	54 17
Palma de Mallorca	3 8·0	1·010	294	290	116	71	37 42	54 6

* Near.

The near coincidence of the eastern half of this track with those of 1860 and 1900 is worthy of note; as also the identity of longitudes between this eclipse and that of 1851, which nearly always results when the 3-Saros period of 54 years 1 month is applied*. Its next return, on the 10th September, 1923, again falls due in America as in 1869; but in the States of the extreme south and west, and with the total phase visible very near, if not actually at, the Lick Observatory.

Observatory House,
Amherst, Mass., U.S.A.,
1905, Feb. 22.

David Todd.
Robert H. Baker.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Discovery of Neptune.

Gentlemen,—

In reading Prof. Turner's recent work, 'Astronomical Discovery,' I was delighted to see, in Chapter ii., "The Discovery of Neptune," that he has ventured to raise again the vexed question of the loss of that discovery to England. It was pleasant reading to me, for having then already been some years employed at Greenwich Observatory, and very familiar with the development of the events leading to the discovery, I have always felt that the outcry that arose against Airy, after the planet was found, was undeserved. His sole remark is that he was "abused most savagely." For although we are all alike liable to mistake, it had appeared to me that in this matter the sins of Airy were in great measure of venial character, as compared with those of others concerned. As a still living witness of the circumstances that then so much impressed me, I had in late years frequently thought of making some statement thereon whilst able so to do, and had collected various information with this view; but to my mind the narration of Prof. Turner puts the whole matter in a very proper light, involving, indeed, statements concerning his old University which it could scarcely have been pleasant to him to make. He, however, speaking with an authority that I could not hope to equal, has removed the necessity for my making any similar attempt. But I might perhaps be allowed to set down some of my own impressions on occurrences as they successively came before me.

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* Mrs. Todd's 'Total Eclipses of the Sun' (London, Sampson Low; Boston, Little Brown & Co.), pp. 247–9.

I remember the application of Prof. Challis to Greenwich, on the part of Adams, for the errors of the tabular places of Uranus, and the prompt sending off to him of what was required. In regard to the two visits of Adams to Greenwich in September and October, 1845, when he was unable to see Airy, owing to causes not necessary here to relate, Airy, after the first visit, wrote to Challis regretting not to have seen Adams, adding that he should be delighted to hear from him; and after the second visit (when Adams had left the celebrated paper of elements of the unknown planet and corrected errors in longitude of Uranus) wrote to Adams himself commenting on the satisfactory results obtained, putting then the question whether the very considerable errors of radius vector of Uranus could be also explained, to which there came from Adams no reply. Airy, in his "Account of some Circumstances historically connected with the Discovery of the Planet exterior to Uranus" (Mem. Roy. Ast. Soc. vol. xvi. p. 397) remarks:—"I waited with much anxiety for Mr. Adams's answer to my query. Had it been in the affirmative, I should at once have exerted all the influence which I might possess, either directly, or indirectly through my friend Prof. Challis, to procure the publication of Mr. Adams's theory. From some cause with which I am unacquainted, probably an accidental one, I received no immediate answer to this inquiry. I regret this deeply, for many reasons." Thus Airy appears to have shown every desire to assist Adams, and anyone who knew him in those days of supreme activity would feel that he would not knowingly neglect any proper duty.

The first thing that occurs to one is, why did not Adams, instead of coming in an inopportune way to Greenwich, make appointment with Airy? On the first occasion he presumably brought Challis's letter of introduction, only to find Airy absent. But why should he, personally unknown to Airy, come a second time in the same unpremeditated and inauspicious way? Had he no friend at Cambridge who could advise him on a better manner of proceeding, especially in a matter so important? And in regard to the radius-vector question, viewing the relation of Airy to Adams, it would surely have been a proper and wise course, assuming even the question to have been an unneccessary one, for Adams to have replied to Airy, especially as the question was of the nature of a test. But Adams having made no serious attempt to see Airy, failed also to answer his letter, conduct that to Airy would appear inexplicable. The extract from Airy's "Account," already given, shows that he was well disposed towards Adams, and ready to promote the publication of his work, on better acquaintance therewith, but at the time he had no real knowledge of its extent and quality, possessing only the before mentioned paper of elements of the unknown planet and corrected errors of Uranus. Again, was there no friend at Cambridge who could see the necessity of bringing these two men together—Airy anxious for more information, and Adams suffering keen disappointment from inability to give it? Had they but met, the quality of Adams's work and the charm of his manner would have captivated Airy and placed him in a position to promote immediate publication of Adams's work.

Airy is supposed at this time to have underrated Adams's labours. But did anyone estimate them at their real value until his results were confirmed by those of Le Verrier, whose action was throughout so thorough, vigorous, and prompt? What, for instance, was thought of them at Cambridge? In Adams's disinclination to answer Airy's raidus-vector question, was there no one to urge him to publish his results? Or, if the work was really thought to be important, why could not the Northumberland telescope have been set to work in the autumn of 1845? For Neptune having come to opposition in the middle of August, although at a south declination of 15°, there should have been fair chance of doing some good work, as, according to Challis's letter of September 22 (that introducing Adams to Airy on his first visit to Greenwich), Adams's calculations were then complete. But faith was wanting, not exactly want of confidence in Adams's work, but rather in the result of an unusual calculation in which various assumptions had to be made. Here is what Prof. Challis himself said (Mem. Roy. Ast. Soc. vol xvi. p. 416):—"It was so novel a thing to undertake observations simply in reliance upon theoretical deductions—while the labour was certain, success appeared to be so uncertain—that it is not surprising a disposition should have been felt to postpone such observations to others based on less speculative grounds."

Then came in succession Le Verrier's memoirs of 1846, June 1, August 31, and October 5 (*Comptes Rendus* of the French Academy of Sciences), confirmatory of Adams's work.

In regard to Airy writing so enthusiastically of Le Verrier's memoir of June 1, it may be asked whether he did more than voice the general feeling on the subject. In Airy's "Account" before quoted we read at pp. 398, 399:—"To this time I had considered that there was still room for doubt of the accuracy of Mr. Adams's investigations . . . . . but now I felt no doubt of the accuracy of both calculations." The action at Cambridge, as shown, had indicated similar doubt. At this moment it was again Airy who, having no fit instrument at Greenwich, or rather the Northumberland equatoreal being a so much more suitable instrument than he possessed, urged Challis on July 9 to search for the supposed planet, offering, if necessary, to send him an assistant from Greenwich, adding, in a further letter of July 13, that "the importance of this inquiry exceeds that of any current work." Challis, however, undertook to sweep for the planet himself without the proffered help, and commenced work on July 29, but appears not to have at once compared together the several nights' work, and so missed detecting the planet which, when its discovery in September was announced, he had himself twice already observed. I have a lively recollection of the vexation of Airy when this became known, and the reason for it. His vigorous action when any important piece of work was in actual progress would not have permitted him to lose a single opportunity of making any necessary comparison of results. But what must have been the feelings of Adams at this further misadventure?

Viewing the whole matter I think it may reasonably be said that Airy's action was not unworthy of him. But the feeling that was at the time aroused seemed unduly to overshadow him for the remainder of his life. In making this slight personal contribution to the history of the subject, I have endeavoured to support what has been said by documentary evidence, and hope in so doing that, as one long employed under Airy, I may not have shown any undue bias.

With reference to the disinclination shown at Cambridge to search for the planet at the opposition of 1845, a similar indisposition appears to have existed on the Continent at the opposition of 1846, after the publication of Le Verrier's memoir of June 1 of that year. A position was created on the Continent somewhat similar to that which had existed at Cambridge in 1845—that is to say, Le Verrier's unconfirmed investigation only was known to continental astronomers, as at Cambridge in 1845 they possessed only that of Adams. Le Verrier in his following memoir of August 31, 1846, pointed out that the opposition of the planet having occurred on August 19, the time was "très favorable" for discovery of the planet, which, by his reasoning, he considered would be distinguishable "par l'amplitude de son disque." But even yet, no continental astronomer had apparently taken up the work. That Dr. Galle found the planet on September 23, on the first evening of searching for it, was due to a direct communication to him from Le Verrier, and in consequence of employing a new star-chart not possessed by English astronomers.

Yours faithfully,
Blackheath, 1905, Feb. 20. William Ellis.

[As Mr. Ellis is the only person now living who had personal knowledge of the circumstances as they appeared at Greenwich, it seems advisable that his aspect of the case should be put on record, but we do not wish any further correspondence on this subject.—Eds.]

Peters' Catalogue.

Gentlemen,—

May I ask you kindly to print the enclosed letter from Prof. Newcomb concerning a matter on which I commented in your columns last July? With regard to the opening sentence, I should like to add that I am entirely responsible for the length of the interval separating the appearance of this letter from that of the comments. Prof. Newcomb had written a previous letter some months ago, but I wished to lay before him certain important facts, viz. that I had seen a letter* from Peters to Mr. Knobel, and that Mr. Knobel had laid his difficulties before Professor Newcomb himself in conversation on the day following the Centenary of the Royal Institution. As Professor Newcomb has not alluded to these facts, I hope I may here re-state them.

University Observatory, Oxford, Yours faithfully,
1905, Feb. 16. H. H. Turner.

1620 P Street, N.W., Washington, D.C.,
February 6, 1905.

Dear Professor Turner,—

Circumstances with which you are acquainted have led to a long delay in my being able to frame a definite reply to your letter on the subject of a passage in my 'Reminiscences' which seems to you unjust to Mr. E. B. Knobel. I also have in mind, in this connection, the remarks on the same subject in the *Observatory* last July. The gravamen of the case is found in my remark:—"No one, so far as I know, ever heard of Peters making any allusion to him or any other collaborator. He seems always to have spoken of the work as his own." These sentences seem to you to contain an accusation against Mr. Knobel of setting up claims to a collaboration which Peters had never recognized.

I certainly never intended any such reflection as this upon Mr. Knobel. The only legitimate conclusion which I supposed could be drawn from the statement was that Peters either failed to give due credit to Mr. Knobel, or did not regard him as a co-author. Now, looking back at the matter, in the cold light of time, I frankly admit that the statement was far from felicitous, and that it was especially inappropriate to raise such a question on the proceedings of a man who had passed away. In this connection you spoke of vaguely including the Royal Astronomical Society in my criticism, because I mentioned Mr. Knobel as a well-known member of that body. The fact is, as I now recall, I felt that there was some possibility that a reader unacquainted with Mr. Knobel's high standing might put too unfavourable a construction on what I said, so I added his relation to the Society to guard against any such inference of the kind.

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* [We have reason to know that Peters' letter to Mr. Knobel referred to contained the following sentence: "through your exertions alone I have been enabled to take into account also the sources that are found in England, and among them many with valuable contributions"; and, further, that, in the interview with Prof. Newcomb, Mr. Knobel definitely offered to present the whole work to the National Academy at Washington.—Eds.]

I may add, what I did not deem it necessary to state in my book, that I was in Clinton shortly after Peters' executors received Mr. Knobel's letter, and that they consulted me about it. I expressed my great surprise at the claim, but assured them that the writer of the letter was a man of high standing in scientific circles. I fancy that they might have hesitated in handing over the manuscript had it not been for this assurance.

In conclusion, to speak with perfect frankness, I did think that there was a seeming anomaly in the proceeding, making it incumbent on Mr. Knobel to do his best toward bringing the work to completion. Nor would I wholly disclaim the conclusion that an explanation of the delay, such as was given in the *Observatory*, would be appropriate. But I did not mean to impugn Mr. Knobel's good faith in the whole matter.

Yours sincerely,
SIMON NEWCOMB.

Professor H. H. TURNER,
University Observatory,
Oxford, England.

The Solar Eclipse of March 16, 1485.

GENTLEMEN,—

In your number for May 1902 (vol. xxv. p. 196) I alluded to the statement made by some writers that a solar eclipse took place on the day (1714, August 1, O.S.) of the death of Queen Anne, and pointed out that this was founded on a misunderstanding of a letter from Swift to Pope, and that no eclipse was possible on the date in question, as the Moon was new two days before, and, moreover, there was no eclipse visible in Europe during the whole of that year.

On the following page, in a letter on a "Comet referred to by Milton," I mentioned that some of the astrologers of that time considered that the death of the Queen Consort of James I. (commonly called Anne of Denmark) was portended by the appearance of the great comet of 1618, although it ceased to be visible in December, and the Queen did not die till the following March.

But I should like now to call attention to an eclipse which really did take place on the day of the death of a Queen of England, also named Anne, and the Consort of the last of the Plantagenets, Richard III., who was vehemently suspected of having poisoned her. For the record of the eclipse having occurred on the day, we must refer to "Historiæ Croylandensis, Continuatio," as published by Dean Gale in the first volume of his

'Rerum Anglicarum Scriptorum Veterium' (Oxford, 1684). It says:—

"Circa medium *Martis* sequentis, in die magnæ ecclipsis [*sic*] solis, quæ tunc temporis accidebat, obiit præfata *Anna* Regina et sepulta est apud *Westmonasterium*, non cum minore honore quam sicut Reginam decuit sepeliri."

The day was the 16th of March, 1485; the eclipse was total in the south of Europe, and, according to Oppolzer's 'Canon der Finsternisse,' the central line, after traversing the Atlantic Ocean from South America, passed over the Spanish Peninsula and North Italy.

Yours faithfully

Blackheath, 1905, Mar. 1.

W. T. Lynn.

Nebular Photography—a Suggestion.

Gentlemen,—

May I be allowed to call the attention of photographic astronomers to a promising field of work which is practically unexplored? I refer to the *planetary* nebulæ (♅'s class IV.), which, from their small angular diameter, cannot be distinguished from stars on the scale of the negatives taken with the "Starfield" reflector, of 98 inches focal length. Although these objects were conspicuously distinct from surrounding stars in the eyepiece of the guiding-telescope (magnifying about 130 diameters), yet on development it was very difficult to pick out the nebulæ on the negatives, so closely did they resemble ordinary stellar images, even with very short exposures. An attempt was made, some years ago, to utilize the principle of the Barlow lens in dealing with them—a negative lens being specially constructed which enlarged the image about three diameters in the primary focus; but it did not answer the anticipations formed of it, as the definition was anything but satisfactory, so it was abandoned. That these planetary nebulæ are, many of them, most interesting objects, we know from the great number of striking drawings made by Sir John Herschel, Lassell, Lord Rosse, and other observers, and it does seem a pity that photography cannot lend its powerful aid in so promising a field of research. One great point in favour of the would-be investigator is that they are usually highly *actinic* in their light, so much so that I have found it required but a few minutes' exposure to secure dense images. A *long*-focus instrument is, however, most essential, and there are such instruments available, such as the photographic objectives of the Lick and Yerkes great telescopes, the Paris *equatorial coudé*, and others. The writers of future up-to-date astronomical text-books might then enrich their pages with many beautiful and unique illustrations of these planetary nebulæ, which as yet are unknown on the photographic plate.

Yours very truly,

W. S. Franks.

Starfield Observatory,
Crowborough Beacon.
1905, March 14.

Great Red Spot on Jupiter.

Gentlemen,—

The following are a few eye-estimated times of transit of the great red spot observed here. Up to August 2 the telescope used was a 10-inch reflector, powers 312 and 332; after the date mentioned a 4-inch Cooke refractor was employed with powers of 150 and 200:—

Date.		Transit-time. h m	Longitude. °
1904	Feb. 2	4 52½	36·0
	June 15	16 9	32·9
	July 12	13 25	29·3
	Aug. 2	15 47	30·6
	Oct. 29	8 9	26·4
	Dec. 18	9 24	25·7
1905	Jan. 12	5 10	27·4
	26	6 48	25·9
	31	5 57	25·4
	Feb. 2	7 36	25·3
	16	5 8	26·5
	19	6 48	27·0
	Mar. 22	7 34	25·3
	27	6 43	24·6

The longitudes indicate that during the interval from 1904 February to June, when Jupiter was near the Sun, and afterwards up to August, the motion of the spot increased, while during the nine months from August to present time the motion was in exact conformity with the rate of System II. (rotation-period $9^h\ 55^m\ 40^s{\cdot}63$).

The spot continued very faint, and in unfavourable conditions was quite invisible. With good definition, however, its outlines could be distinctly traced in the 10-inch reflector. When the air was unsteady, transits of the centre of the hollow in the great southern equatoreal belt were taken instead of the spot, as the latter is placed centrally south of the hollow.

Since the summer of 1894, the mean motion of the red spot has caused it to lose only 27° of longitude, so that the rotation-period has been $9^h\ 55^m\ 40^s{\cdot}9$, and only a quarter of a second greater than the period adopted in the Marth-Crommelin ephemerides during the last eleven years.

Pretty frequent observation of Jupiter with a 4-inch refractor of first-class quality has shown that, however good an instrument of that aperture may be, its light-grasp is insufficient for the really effective study of the most delicate features on this interesting planet.

Yours faithfully,

W. F. Denning.

Bishopston, Bristol,
1905, March 30.

Tempel's First Periodic Comet.

[Translation.]

GENTLEMEN,—

I have read with much interest in the March number of your Magazine (p. 142) the note by Mr. W. T. Lynn on Tempel's First Periodic Comet (1867 II.). I understand that the probability of this comet being re-observed diminishes at each of its returns. However, I do not wish to let that of 1905 pass without a search, because the opposition happens this year under better conditions of observation than the preceding one in 1898.

The least distance from the Earth will be 1·11 at the beginning of June, and as the comet has actually a perihelion distance of 2·07, one cannot hope for more favourable conditions. I have therefore computed an ephemeris from the elements of 1898 * reduced to the year 1905, which will be published in a subsequent number of the *Astronomische Nachrichten* †.

I shall be very much obliged to all those who are willing to use it and will devote a little time to a search for this interesting comet.

With kind regards,
Yours very truly,
R. GAUTIER.

Geneva Observatory,
1905, March 23.

NOTES.

COMET NOTES.—The ephemerides for Comet 1904 II. (Giacobini) and 1904 *e* (Borrelly) are continued. They are for Berlin midnight. The brightnesses on May 4 are 0·21, 0·12 of that at discovery:—

	1904 II.		1904 *e*.	
	R.A.	N. Dec.	R.A.	N. Dec.
	h m s	° ′	h m s	° ′
Apr. 2......	23 39 58	65 5	5 5 32	42 34
6......	23 56 32	65 9	5 19 44	43 19
10......	0 12 17	65 12	5 34 6	43 56
14......	0 27 13	65 12	5 48 33	44 26
18......	0 41 22	65 11	6 3 3	44 50
22......	0 54 47	65 8	6 17 32	45 6
26......	1 7 30	65 6	6 31 57	45 16
30......	1 19 33	65 3	6 46 15	45 20
May 4......	1 30 58	65 0	7 0 24	45 17

Ast. Nach. 4002 contains an interesting article on the physical appearance and variations in brightness of Comet 1904 I. The nucleus seems to have been about 5000 kil. in diameter, the coma about 200,000 kil. The brightness diminished from mag. 9 in April 1904 to 11½ in September.

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* *A. N.* vol. cxlvi. p. 177, No. 3492, 1898 March 9.

† The probable position of the comet on April 2 is R.A. $17^{\mathrm{h}}\ 23^{\mathrm{m}}$, S. 18½°; April 30, R.A. $17^{\mathrm{h}}\ 41^{\mathrm{m}}$, S. 21°.

A faint comet was discovered by M. Giacobini at Nice, its place on March 26·3212 G.M.T. being R.A. $5^h\ 44^m\ 14^s$, N. Dec. $10^\circ\ 56'\ 56''$. A later observation by Prof. Aitken at Lick gives the daily motion $+3^m\ 28^s$, N. $1^\circ\ 13'$. A. C. D. C.

MINOR PLANET NOTES.—The following planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Mag.
QD	1905 Mar. 8	Wolf.	12·5
QE	13	Götz.	12·5
QF	13	Wolf.	12·2

Planets PX, QC, announced last month, prove not to be new, but identical with 517, 427, respectively.

The following planets have received permanent numbers: PK 549, PL 550, PM 551, PO 552, PP 553. PG, PJ, PN, do not receive permanent numbers owing to insufficient observations. PH is probably 157 Deianira. Planet 526 is very probably identical with HA, 550 is identical with FT and also with LX.

Ast. Nach. 4005 contains an ephemeris by Prof. Kreutz of 475 Ocllo, whose orbit has a very large eccentricity. It appears that the objects observed for this planet in 1903 were not really identical with it. Its recovery at the forthcoming opposition is therefore greatly desired. A. C. D. C.

OBITUARY.—PIETRO TACCHINI. With much regret announcement has to be made of the death, which happened on March 19, of this distinguished solar observer. Pietro Tacchini was born at Modena on March 21, 1838. After taking his degree at the University of that city, when nineteen years of age he took up the study of astronomy and so came under the notice of Father Secchi, then the Director of the Observatory of the Collegio Romano. Through Secchi's influence in 1863 he was appointed Assistant at the Observatory in Palermo, and here his talent as a draughtsman found opportunity, for it was his task to make daily drawings of the Sun with its spots and prominences. Later he took up meteorology and other work pertaining to the Observatory, and subsequently was made Director at Palermo. In 1878 Father Secchi died, and for a time the Observatory of the Roman College was directed by Father Ferrari, but the political question arose and, the priests being turned out of the public offices, Prof. Tacchini was asked in 1879 to take charge of the Observatory, which was then made the Headquarters of the Meteorological Office of Rome. After his appointment he devoted much attention to the observation of sun-spots and prominences, visual and spectroscopic, publishing his results in the 'Journal of the Spectroscopic Society of Italy,' an organization which he had founded in 1872. In 1883 he accompanied M. Janssen to Caroline Island, in the Pacific, for the observation of the total solar eclipse of May in that year; he also went with Sir Norman Lockyer's party to Grenada, in the West Indies, to observe the eclipse of 1886 August, and on this occasion he ascertained some significant facts about "white" prominences, viz., that only the cores of these objects are seen by the spectroscope under ordinary circumstances, but that their upper sections, which give a faint continuous spectrum, can only be seen when the scattered light which prevents their observation is withdrawn during an eclipse. Other notable work of Prof. Tacchini is his series of observations of Venus, which led to a result confirming Schiaparelli's conclusion that the planet rotates on its axis in 225 days—the period of revolution.

Prof. Tacchini was elected an Associate of the Royal Astronomical Society in 1883 November. In 1888 he received the Rumford Medal of the Royal Society for solar work, and in 1892 the Janssen Gold Medal of the French Academy of Sciences. He resigned the Directorship of the Observatory of the Collegio Romano in 1902.

ROYAL METEOROLOGICAL SOCIETY'S EXHIBITION.—An exhibition of Meteorological Instruments, arranged by this Society, was held from March 14 to 17 in the Library of the Institution of Civil Engineers, Great George Street, Westminster.

The chief feature was the development of recording instruments and also the methods adopted for ascertaining the meteorological conditions in the upper air. The instruments used by Mr. J. Glaisher, F.R.S., in his famous balloon ascents for the British Association, 1862–66, were placed in position on the board as used in the car of the balloon. Mr. W. H. Dines showed the kites and meteorograph used by him at Crinan, and Mr. W. S. Bruce showed the camera and meteorograph used with kites in the Scottish Antarctic Expedition. The thermometers, anemometers, and sunshine recorders used on board the 'Discovery' in the recent National Antarctic Expedition were also exhibited, as well as a number of beautiful water-colour drawings of sky and cloud effects in the Antarctic regions made by Dr. E. A. Wilson. Dr. J. L. Thomas exhibited a life-size "dummy" dressed in the remains of the clothes of a man who was struck by lightning near Cardiff on July 17th, 1903. M. Richard, of Paris, showed a collection of most of his noted self-recording instruments, and Mr. Halliwell, of Southport, exhibited his new rain-gauges, anemometers, and evaporation-tank. The Royal Meteorological Society arranged in a railed-off enclosure a typical climatological station. The Meteorological Council also showed, in position, the photographic barograph and the Beckley self-recording rain-gauge which were recently in use at the Fort William Observatory. Many other instruments were shown, as well as a large collection of photographs, records, drawings, maps, &c., illustrating meteorological phenomena.

THE PARIS—GREENWICH LONGITUDE.—As a complement to some determinations of longitude made in the last few years, Prof. Albrecht has made a "compensation" of European longitudes (*A. N.* 3993–94). Having tabulated the results of, and assigned weights to, 176 longitude determinations, beginning with Brest—Paris made in 1863, and ending with Greenwich—Potsdam of 1903, he has combined these and deduced the final longitude measured from Greenwich of 95 stations on the Continent of Europe. The longitude of Paris (meridian of Cassini), as it appears in this final table, is $9^{\rm m}\ 20^{\rm s}{\cdot}932$, which happens to be the exact result found by the English observers in 1902, but the accordance of the third decimal cannot be regarded as anything but accidental. This seems, however, to be the last word on a subject long under discussion; and the moral seems to be that, twenty years ago, a tenth of a second of time was not too large an error to expect in a longitude determination.

PROF. TURNER gave a series of three afternoon lectures at the Royal Institution on March 2, 9, and 16, which, judging from the comments overheard on the staircase afterwards, appeared to give general satisfaction. The series was entitled "Recent Astronomical Progress," and in the first the lecturer, dealing with solar matters, explained recent developments of the Sun and Earth-magnetism question, and described the new Observatory on Mount Wilson and its proposed work. The second lecture was devoted to an explanation of the lunar theory, a difficult subject to make clear in a popular lecture, but the difficulty was successfully surmounted. On this afternoon the discovery of new satellites was brought before the audience in the manner described on p. 195 of this number. On March 16 the Savilian Professor spent some time in giving an account of the work at some American Observatories which will be familiar to readers of this magazine, and in showing photographs of nebulæ. Descriptions of these, of variable stars, and orbits of binary systems led up to the suggestion of the existence of dark obscuring matter in the Universe.

WEATHER IN MARCH.—The past month has been notable for an excessive rainfall. March is usually the driest month of the year, the Greenwich 60-years' average giving a total of 1·48 inches; but the fall during the third month of this year at Greenwich has amounted to 3·55 inches, which makes it the second wettest March since the beginning of the official record in 1841. The actual wettest was March 1851, when the total amount was 4·05 inches. Almost the whole of the rain this year fell in the first half of the month.

OWING to pressure on our space we are compelled to hold over notices of several interesting works—Mr. Whittaker's Dynamics, Count de Miremont's Star-maps, and others.

Starfield, Crowborough Beacon, the house and observatory of the late Dr. Roberts, was put up to auction on March 17, but was not sold. The highest bid was £5000, which was evidently below the reserve.

The next Meeting of the Royal Astronomical Society will be on Friday, April 14; of the British Astronomical Association on Wednesday, April 26.

From an Oxford Note-Book.

Amateur astronomers, and others possessed of small telescopes, are so often enquiring what useful work they can do, that no apology is needed for again calling attention to a field of work specified by that keen observer Professor E. E. Barnard more than 20 years ago (Sept. 20, 1882), which has not hitherto attracted many labourers, so far as I know. When a bright meteor leaves a train which remains visible for a few seconds to the naked eye, a small telescope can probably follow the train for some minutes; and careful observations of its drift among the stars would give valuable information as to the currents in the upper atmosphere. Professor Barnard's paper appeared originally in Vol. I. of the *Sidereal Messenger*, giving particulars of a train observed in November 1881, and of 6 others seen August 5–19, 1882. It was reprinted in 1891 by request. But as many readers may not even yet have seen it, I will reproduce verbatim the concluding paragraph:—

> To those beginning such observations it may be well to say: Note the direction of flight of the meteor; turn the telescope at once to its path, sweep rapidly back and forth over the place where the meteor passed. If there is a train left you will likely strike it the first sweep. If bright and persistent, sweep the full length of it, examining any peculiarities; then let your telescope stand at rest and allow the train to pass across the field, note the time it requires to pass through from edge to edge of field, taking into account the motion of the sky, note the direction of motion, the width of the train, the time of observation within a few minutes, note the point of observation in the sky, &c. When the train begins to bend or become irregular, which it is sure to do if it remains visible any length of time, see if the forward portions (in the direction of motion) are brighter than the general train. It would be well to watch the stars it passes over, also to note if there is any change in steadiness and brightness as the luminous mass passes over them; write out your observations and send them to the *Sidereal Messenger* or some other scientific journal where they will receive proper attention. If several observers some distance apart could attend to this kind of work and carefully record the paths of the meteors seen, it is likely that the same object might be examined by two or more, and its height above the earth could be determined, and therefore the actual velocity with which its train moves and the quantity of gaseous matter it contains.

If an excuse be wanted for again calling attention to this field of work I may find it in the fact that bright meteors are sometimes observed without study of their trains at all. Take, for example, the following record, where it is clear that the observer was more anxious to note the exact time than to scrutinize the fine train which must undoubtedly have existed, and which he does not even mention:—

> Last night a most wonderful fireball was seen at this station. While walking around outside the Observatory "between stars" of the latitude observing programme, a fireball of the shape of the crescent Moon appeared suddenly in the south-east, moved slowly to the north in a horizontal plane about on the almucantar of the South Pole. As the object moved it developed rapidly, passing through phases like the Moon until the full-moon stage was reached. After remaining at this maximum size a moment, this remarkable object then passed rapidly through the phases of the waning Moon, and finally disappeared about ten degrees east of the Pole Star. When full the fireball was somewhat larger than the full Moon—perhaps about 37′ in diameter—but not quite so bright as the Moon. When in the crescent form just before disappearance the whole disk was seen faintly appearing like the new Moon when lit up by the earth-shine. Immediately after the disappearance of this remarkable fireball I went into the observatory to record the time, which was found to be 1^h 40^m, and then I awoke with a dull, sickening thud, and found it was quarter past seven, and time to get up and light the fire. (Pub. Ast. Soc. Pac. xvii. p. 34.)

The last sentence of the above extract allows us to infer the date of this remarkable occurrence, which is wrongly specified: for at 7.15 A.M. it was apparently 1^h 40^m local sidereal time. Hence it must have been *not* December as stated, but the end of June, and consequently the meteor appeared in broad daylight, if we are right in ascribing the events to the northern hemisphere: which make them still more remarkable. A base suggestion has been made that the narrative is not to be read quite seriously; but I hesitate to believe that anyone would venture to ruffle the still depths of astronomical record with the breath of levity. There is at least this to deter him—that his evil example might be followed.

FROM a correspondent:—

Ideas on Astronomy of la Duchesse de Ferte in the early part of the 17th century: extracted from 'The French Noblesse of the Seventeenth Century':—

> "Some foolish persons," said she, "on seeing falling stars believe that they are souls wending their way to God. Not at all. They are Princesses being born. Never forget this, my child."
>
> On my way home I did not fail to relate this to the others.
>
> "Really," said my grandmother, "your aunt was rather sensible to-day. The other morning she told little Mlle. du Chatillon that the Moon was a black hen, and was not bigger than a cooking-pot."

A CATALOGUE of 1440 zodiacs and planispheres has been published by the Rev. A. B. Grimaldi, of Caius College, Cambridge (London: Gall & Inglis, 25 Paternoster Square). The collection of the material must have taken much labour; but can such a catalogue ever approach completeness? I see the terra-cotta figures on the Royal Observatory are catalogued (No. 1239), but I fail to find mention of the Radcliffe Observatory, Oxford. Perhaps this is merely an oversight on my part, for there is no obvious arrangement of the Catalogue, so that one has to read the whole list to find anything. No. 1235 seems interesting, "Twelve designs of the Signs as Golf-players" (*Black and White*, 8 December, 1894).

The list of Chinese zodiacs must be very incomplete—only six; and Japan is not mentioned. But there are some fine zodiacal figures round one of the temples at Nikko, of which I had a series of photographs taken:—rat, bull, tiger, rabbit, dragon, serpent, horse, goat, monkey, cock or eagle (certainly not *stork*), dog, hog. The tiger is a beauty and has a wide smile, like the tiger of romance who had "the lady inside."

At a lecture at the Royal Institution the other day the following table was put upon the screen as a nucleus for remarks on the discovery of new satellites:—

Date.	M.	J.	S.	U.	N.	
To 1685......	...	4	5	...	...	Foreign.
1787–9	...	...	2	2	...	England.
1848	...	...	1	...	...	America.
1846–51......	...	...	...	2	1	England.
1877	2	...	...	...	...	America.
1892	...	1	...	...	...	America.
1899	...	...	1	...	...	America.
1904	...	1	...	...	...	America.
Score:—England 7. America 6.						

It was remarked by the lecturer that since the slide had been prepared the addition of a 7th satellite to Jupiter by an American discoverer had brought the score "all-even," and that England was on the point of being passed in the race. The daily papers showed some interest in this contest (pending future yachting arrangements by Sir Thomas Lipton), as the following extract from the *Daily News* of March 10 will indicate. Perhaps it will only be kind to our American brethren, including the discoverer himself, to explain that the telegram announcing the discovery of J. VII. reached us in Europe from Kiel *with no name attached*; and the confusion of the reporter in regarding the Kiel announcement as a new one, unknown to the lecturer, is thus pardonable:—

> Are there "fashions" in astronomy, as in lesser matters? The discovery of new planetary satellites seems to be the order of the day. But a few weeks after the finding of Jupiter's sixth Moon, a telegram from the Observatory at Kiel announces that a seventh, of the sixteenth magnitude, has been seen.
>
> Particulars have yet to reach us. It may turn out that this little world is one of the supposed companions of the fifth satellite, according to the suggestion that the plane in which that body moves may be the arena of numberless fragments of a shattered Jovian Moon.
>
> This Kiel discovery, by the way, introduces Germany into the satellite-hunting match which our own country and the United States have been holding

lately; each at present having, as Professor Turner pointed out in his lecture at the Royal Institution yesterday, seven satellites to its record.

And Russia, there is reason to believe, has been seeing stars constantly since February 1904.

The headings of the paragraphs referring to the matter generally included some reference to a "dead-heat between England and America," and the *Star* made rather a good joke on the subject. It pointed out that when it comes to *dead*-heat, the Moon is "an easy first."

THE following sentence from 'The Mill on the Floss,' Book vi. Chap. xii., opening paragraph, is rather difficult to construe. George Eliot is, in general, so careful a writer that one looks for some slip of the pen which may have played havoc with a fine metaphor; but I find it difficult to suggest a single emendation which will bring the passage into complete accordance with known facts. Perhaps some one else may hit upon the clue:—

> . . . . a change in the fortunes of the Tullivers, which was likely finally to carry away the shadow of their demerits like the last limb of an eclipse, and cause their hitherto obscured virtues to shine forth in full rounded splendour.

FROM a correspondent:—

In Victor Hugo's 'William Shakespeare,' Part ii. Book i. Son Génie, speaking of the far-reaching influence of imagination, we read as follows:—

> La science, arrivée aux derniers abîmes, la rencontre. Dans les sections coniques, dans les logarithmes, dans le calcul différentiel et intégral, dans le calcul des probabilités, dans le calcul infinitésimal, dans le calcul des ordres sonores, dans l'application de l'algèbre à la géométrie, l'imagination est le coefficient du calcul, et les mathématiques deviennent poésie. Je crois peu à la science des savants bêtes.

Thus, in the eyes of a poet, figures are not the dry bones that some people think.

FROM the *Liverpool Daily Post* of January 27:—

> In the current issue of *St. Bartholomew's Parish Messenger* (Naylor Street) the Rev. John Howard instructs his parishioners in the inner meaning of the "Song of Sixpence." He says:—"Perhaps many who often repeat 'Sing a Song of Sixpence' have never heard this explanation of its meaning:—The four-and-twenty blackbirds represent the four-and-twenty hours. The bottom of the pie is the world, while the top crust is the sky which overreaches it. The opening of the pie is the day-dawn, when the birds begin to sing; and surely such a sight is fit for a King. The King, who is represented as sitting in his parlour counting his money, is the Sun; while the gold pieces which slip through his fingers as he counts them are the golden sunshine. The Queen, who sits in the dark kitchen (? pantry), is the Moon; and the honey with which she regales herself is the moonlight. The industrious maid, who is in the garden at work before the King—the Sun—has risen, is day-dawn, and the clothes she hangs out are the clouds; while the bird, who so tragically ends the song by 'nipping off her nose,' is the hour of sunset. So we have the whole of the day, if not in a nutshell, in a pie." Our old friend the Sun Myth once more! His uses are inexhaustible."

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1905 April 14.

Mr. W. H. Maw, *President*, in the Chair.

Secretaries: E. T. Whittaker, M.A., and
T. Lewis.

The Minutes of the previous Meeting were read and confirmed.

Mr. Lewis. Seventy-one presents have been received since the last Meeting, amongst them are included a copy of Prof. Turner's new book, 'Astronomical Discovery,' presented by the author; nineteen enlargements of Astrographic Chart plates from the Royal Observatory, Greenwich; eighteen copies of similar enlargements from the San Fernando Observatory; and a volume of Sydney Observatory Observations, 1862, presented by the Observatory.

The President. At our last Meeting I had the pleasure of announcing that Prof. Turner had kindly undertaken to deliver an evening lecture to us during this month or in May. I am now able to say that the date fixed is Wednesday, May 3, and the time 8.30 P.M. The subject of the lecture will be "The Coming Eclipse."

Mr. Conrady gave an abstract of a paper on "The Spherical Aberration of Object-glasses," in which he dealt with the subject from the physical point of view. He pointed out that the usual geometrical treatment of spherical aberration as a *confusion* of the rays meeting at the focus was unsatisfactory, inasmuch as the undulatory theory made it quite clear that the *phase-relation* of the light arriving at the focus was the important feature and that aberrations should therefore be expressed and measured as differences of phase between the rays instead of being characterized by deviations from their desired direction.

With the aid of diagrams thrown upon the screen he first showed that considerable knowledge of spherical aberration in this sense might be derived from the known laws of geometrical spherical aberration. He then proceeded to explain two methods, one being algebraical, the other trigonometrical, which led to rigorous equations, by which the differences of phase between selected rays passing through any lens-system might be computed; the second of these equations was discussed, and a chromatic condition derived from it by differentiation with regard to the wave-length. In conclusion he showed how a relation between the usual geometrical aberration and the phase-relation at the focus could be deduced from his equations, and pointed out that this relation was such as to prove that it was far easier to bring all rays to a focus with differences of phase not exceeding a quarter of a wave-length (that being a limit suggested by Lord Rayleigh) than to concentrate them within a circle of confusion not larger than the spurious star-disk.

Mr. Lewis and *Mr. W. W. Bryant* asked questions relating to the formulæ shown on the screen, which Mr. Conrady answered. Also, in reply to *Major Hills*, he further explained Lord Rayleigh's suggestion mentioned above.

The President. I am sure you will all return your thanks to Mr. Conrady, not only for his paper, but also for the lucid manner in which he has put this rather technical matter before us.

Mr. H. C. Plummer. Before referring to my paper ("Note on Point Distributions on a Sphere, with Remarks on the Determination of the Apex of the Sun's Motion"), I should like, with the President's permission, to show a slide which illustrates a suggestion made in my recent paper on the coelostat. This shows the coelostat with a second mirror to reflect the rays down a long vertical shaft in the ground, at the bottom of which is a concave mirror. For a telescope of great focal length (100 feet or more) it has been usual, as in the Snow telescope, to place the axis nearly parallel to the ground. This has two disadvantages: one is that the focal length is liable to great variations owing to changes of temperature, and the other is the bad definition owing to air-tremors near the ground. With the vertical arrangement the temperature of the concave mirror may be expected to be more nearly constant, and the atmospheric disturbances may quite possibly be less injurious.

The point distributions on a sphere, to which my paper refers, are such as show a zone of condensation, the problem being to determine the position of the pole of the zone. The distribution of stars in the sky is an illustration. The solution is found by attributing equal masses (or, if necessary, a system of different "weights") to the points, and calculating the momental ellipsoid. The axis about which the moment of inertia is greatest has the direction of the required pole. This is the mechanical interpretation of the method employed by Prof. Newcomb in a recent memoir to determine the position of the galactic pole. A second use of the method is found in the determination of meteor-radiants, the points which show condensation in this case being the poles of the tracks. A third example is the method suggested by Bessel for the determination of the apex of the Sun's motion. Here the points are the poles of the proper motions of the stars. This method has been extensively used by Dr. Kobold. Now determinations by other methods have placed the declination of the apex between +20° and +45°, while Dr. Kobold places it practically on the equator. This marked discrepancy requires examination. Dr. Kobold's application of Bessel's method was criticised by M. Radau. He pointed out the distinction between the direct proper motions (with components away from the apex) and the retrograde motions (with components towards the apex). Dr. Kobold, by ignoring this distinction, had virtually reversed the latter, and M. Radau explained the discrepancy by supposing that the apex indicated by the direct motions differed from that indicated by the retrograde reversed in direction by some 60°. Now if this be true we cannot stop here. The assumption on which determinations of the apex are based is that the intrinsic proper motions are distributed accidentally. It follows that the momental ellipsoids corresponding to the two sets of motions should be ellipsoids of revolution and their axes of symmetry should coincide. If we must conclude that their axes are widely separated, we must abandon the fundamental assumption of random distribution. Merely to render definite the idea of random distribution we may compare the Sun to a molecule in a mass of gas whose molecules obey approximately the Maxwell-Boltzmann law. The departure from this state of things, of which we must contemplate at least the possibility, may be compared with the crossing of two or more independent streams of gas. This conclusion cannot be regarded as original, for the absence of a really accidental distribution of intrinsic proper motions is implied in the progression with star-magnitude found by Stumpe in the declination of the apex, and by Prof. Campbell in the motions in the line of sight.

Mr. Franklin-Adams. I would like to make one short suggestion. At Prof. Winkler's Observatory at Jena, for some purpose a well has been cut in the sandstone rock. What experiments this has been made for, or whether they have succeeded, I do not know; but there certainly is a well cut 80 feet deep, and there is a circular staircase cut by its side. I would suggest that Mr. Plummer might write to Prof. Winkler and ask whether any experiments have been made as to stopping these air-tremors. Perhaps these might be done away with altogether or partly by moving the air with an electric fan, but it would be well to find out what experiments have been made at Jena.

Mr. Dyson. Bessel's method of finding the solar apex is interesting theoretically, but is open to some serious objections which ought to be borne in mind before accepting unreservedly conclusions based on it. It does not seem to me to be entirely a question of separating the direct from the retrograde proper motions. Account should be taken of the magnitude of the proper motions. If we consider any small area of the sky, each of the smaller proper motions, which tend (generally speaking) away from the apex, determines one point with just as much weight as the larger ones, which tend (generally) towards the apex; the magnitude and sign of all or any of the proper motions can be changed without altering the result. The methods of Airy and Kapteyn are much more reliable.

Prof. Turner. At the Conference at St. Louis last summer, where I had the honour of representing the Society, Prof. Kapteyn gave us an address on the solar motion and referred in the first instance to the point Mr. Dyson has raised—as to the difference in magnitude of other proper motions that the solar motion itself makes. He went on to say that certain discrepancies in the deduced position of the apex, according to his figures, meant that we are in the presence not of one system of stars but of two systems. That is my recollection of the chief point of the paper, which has unfortunately not been published yet. When Mr. Plummer sent me his paper to look at the other day, I recognized an independent and quite simple proof of what I take to be Kapteyn's main point, that we are in the presence of more than one stellar universe. It seems to me that Mr. Plummer puts that very simply in his paper. His method of deducing the direction of solar motion is something as follows:—On the sphere he plots the stars and gives them weight according to the magnitude of their proper motion, and determines the axis about which the sphere would spin without wabble, or, as it is called, the axis of the greatest moment of inertia. Then he points out that if you take the stars with *direct* proper motions and perform this operation, and then the stars with *retrograde* motions, the axes do not coincide—which seems to lead in a very neat way to Prof. Kapteyn's conclusion that we must be dealing with two systems at least.

Mr. Hinks. I have been much interested in Mr. Plummer's account of the use of the momental ellipsoid in problems of star-distribution, and I should like to raise two or three points. It does not seem to me to be legitimate to discuss separately the two groups of stars whose motions with respect to the solar apex are "direct" and "retrograde": in the first place, because the division is arbitrary, without physical significance, since a mere numerical alteration in the speed of the Sun would carry over a number of stars from one group to the other; and secondly, because the effect of the solar motion upon the distribution of the poles of the apparent proper motions is different in the two cases, being in one case a condensation towards the plane of the great circle whose pole is the apex, and in the other case a condensation towards the apex. It seems to me that it might be better to speak of planes of symmetry rather than of condensation ; but it is not then obvious, even if true, that the axis of the momental ellipsoid directed towards the apex is necessarily the shortest for both groups, even if motions are distributed at random. I would also question whether the proposed criterion for random distribution is sufficient, since it is easy to imagine cases of decided systematic but asymmetrical distribution of proper motion not unlike those found by Prof. Kapteyn which would nevertheless have a momental ellipsoid approximately spheroidal.

Mr. Plummer, in reply, said: Mr. Hinks in his last remark has practically suggested the converse of the proposition which I affirmed. I quite agree that such a deduction is not legitimate. Random distribution should lead to a spheroidal form of the momental ellipsoid, but this form can also arise from asymmetric distribution. The other points raised by Mr. Hinks I have not understood or remembered so well as I should have done. But the main thing is that in an accidental distribution (or, more definitely, a statistically symmetric system) the components at right angles to the parallactic motion are balanced, and therefore on the whole should produce no deviation from the direction of the apex. As regards Mr. Dyson's remark about the method, I agree that Bessel's method is not without difficulty in application. Still if the data were satisfactory I think it would give a result not very far from the truth. If, on the other hand, we admit that the data are unsatisfactory, and we have to make "the best of a bad job," then probably some other method, such as Airy's, is more suitable. But I do not think that this affects the general deduction, for the argument is rather qualitative than quantitative*.

Mr. H. F. Newall gave a description, with illustrations on the screen, of the four-prism spectrograph attached to the Newall telescope of the Cambridge Observatory, and made some remarks, with explanatory formulæ and tables on the blackboard, on the general design of spectrographs for equatorials of large apertures considered from the point of view of tremor-discs.

The President. We thank Mr. Newall not only for the very interesting paper he has brought before us, but for the admirable way in which he has condensed the theoretical deductions he has drawn. He has stated that he knows much more about the spectroscope than when he started, and I think that we may add that he has also made us know a good deal more about it. It is exceedingly interesting to examine that table which Mr. Newall has brought before us, and to notice the way in which the cutting out of certain portions of the tremor-disc is compensated for by a reduction of other causes of loss of light. It is very gratifying to find that the question of how best to proportion a spectroscope for use with a given telescope has been taken up so thoroughly in this country. The investigations on the adaptation of the spectroscope to astronomical requirements published in the past few years have been mostly from foreign sources, and I am sure we all congratulate Mr. Newall on the work he is doing at Cambridge, while we may also congratulate ourselves on the fact that we shall soon have a report of the results.

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* Owing to the kindness of Mr. Hinks I have been able, since the meeting, to see the above report of his remarks. He is quite right in pointing out that the momental ellipsoid corresponding to the retrograde motions is possibly a prolate and not an oblate spheroid. This consideration was in my own mind, and I have used the term "axis of symmetry" in preference to "least axis" deliberately in this connection. His other remarks do not seem very relevant to my argument. A more serious objection to Bessel's method seems to me to lie in the influence of the distribution of the stars themselves, and of the systematic relations between the proper motions and this distribution. An idea of the magnitude of these effects could only be gained by a detailed examination of the applications of the method.—H. C. P., April 19.

The following papers were announced and partly read:—

W. F. Denning. Value of Meteoric Radiants based on Three Paths.

G. W. Hough. Determination of Longitude on the Planet Jupiter.

A. Stanley Williams. Revised Elements of UY Cygni.

A. Stanley Williams. Revised Elements of Y Lyræ.

H. H. Turner. Further Note on Instrumental Errors affecting Observations of the Moon; in reply to Mr. Cowell's Paper of June 1904.

P. H. Cowell. Reply to Prof. Turner's Paper.

H. C. Plummer. Note on Point Distributions on a Sphere; with Remarks on the Determination of the Apex of the Sun's Motion.

F. W. Dyson and D. J. R. Edney. Discussion of the Observations of the Satellite of Neptune made at the Royal Observatory, Greenwich, in the Years 1902–3–4.

A. E. Conrady. On the Spherical Correction of Object-glasses.

P. H. Cowell. The Coefficients of the Principal Terms in the Moon's Latitude.

H. F. Newall. Description of a Four-Prism Spectrograph attached to the Newall Telescope of the Cambridge Observatory; and Remarks on the General Design of Spectrographs for Equatorials of Large Aperture, considered from the point of view of Tremor-discs.

The following foreign astronomers were proposed by the Council as Associates of the Society:—

Theodor Albrecht, Königlich-preussische geodätische Institut, Berlin, Potsdam, Germany.

Gustav Müller, Astrophysikalisches Observatorium, Potsdam, Germany.

Rodolphe Radau, Membre de l'Institut, Paris.

The following gentlemen were elected Fellows of the Society:—

William Bowyer, Royal Observatory, Greenwich, S.E.

Rev. Thomas Joseph Charlton, The Rectory, Omeath, Co. Louth, Ireland.

Capt. Louis Arthur D'Emres, Marine and Fisheries Department, Ottawa, Canada.

David James Reginald Edney, Royal Observatory, Greenwich, and Teston Lodge, Blackheath Rise, Lewisham, S.E.

Herbert Henry Furner, Royal Observatory, Greenwich, S.E.

John Adalbert Parkhurst, M.Sc., Yerkes Observatory, Williams Bay, Wis., U.S.A.

Montagu Austin Phillips, 22 Petherton Road, Highbury New Park, N.

A. L. Wood, H.M.S. 'Conway,' Rock Ferry, Birkenhead.

The following Candidates were proposed for election as Fellows of the Society :—

Major D. F. S. Baden-Powell, 32 Princes Gate, S.W. (proposed by W. J. S. Lockyer).

Joseph Henry Elgie, The Grange Avenue, Leeds (proposed by C. T. Whitmell).

Frederick William Longbottom, Haslemere, Queen's Park, Chester (proposed by W. E. Plummer).

Frederic John Mathian Stratton, B.A., Isaac Newton Student in the University of Cambridge, Caius College, Cambridge (proposed by Sir R. S. Ball).

John Willis, late India Office, retired, Merkara, 19 Bouverie Square, Folkestone (proposed by Sir R. S. Ball).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 March 29.

President: A. C. D. Crommelin, in the Chair.

Secretaries: J. A. Hardcastle.
J. G. Petrie.

The Minutes of the previous Meeting were read and confirmed.

The names of seven Candidates were read and passed for suspension, and the election by the Council of three new Members was unanimously confirmed.

A note by *Mr. T. E. Heath* on "Stereoscopic Views of the Stars" was read, and two views sent by the author were handed round for inspection.

The President said that some 13 years ago he sent a similar paper to the Association, and handed round a similar diagram. He did not, however, put in nearly so many stars as Mr. Heath had done. Another difference was that he (the speaker) drew his diagram with a white background on squared paper; the ruled lines greatly helped the eye in throwing the diagram up in relief. He hoped the hint would be extended in other directions, and that they would have stereograms, for instance, of Saturn, showing it standing out in relief from its ring, and that the Sun might be made to appear in relief.

Mr. Hardcastle read extracts from a paper by *Col. Markwick* on "The Great Sun-spot of January and February 1905," accompanied by several views.

Mr. Maunder said he had received a very fine photograph of the great spot sent him by a Danish astronomer, Dr. August Henze, taken with a photoheliograph of 3½-in. aperture. This photograph was shown by means of the lantern.

The President said he believed the photograph just exhibited was taken with a telescope of 3½-in. aperture, and illustrated what Mr. Chambers said at the last Meeting as to the work that might be done on the Sun with a small instrument. The detail shown by the photograph was wonderful; he did not think the Greenwich photograph showed much more.

Mr. Maunder said the photograph taken on the same day at Greenwich showed very much the same amount of detail.

Mr. Hardcastle. What aperture?

Mr. Maunder. We were using only 4 in., so that there was not very much difference between the powers of the instruments.

Mr. Chambers was very glad to hear what Colonel Markwick said in his paper about the possibilities of small telescopes. He took to Portugal, in 1900, a 2-in. Navy glass, with which he obtained a splendid view of the total eclipse.

Mr. Maunder read some letters which he had received from *Dr. A. Reichwein*, of Steglitz, near Berlin, throwing out the suggestion that stereoscopic photographs of the corona should be taken during the August eclipse. If, said Dr. Reichwein, a photographer were stationed in America as well as Spain, both with photographic lenses of exactly the same focus, the two images must, if combined in the stereoscope, give a stereoscopic effect, assuming that the corona did not change in shape. "The stereoscopic method" should therefore give the solution of the following hree questions:—

(1) Does the corona change in 2½ hours?
(2) Does it rotate with the Sun?
(3) What is its shape?

Mr. Maunder said that the suggestion contained in the letters was not exactly a new one, but the practical difficulties in carrying it out would probably be considerable. There was, however, no need to wait until the forthcoming eclipse. There were a large number of coronal negatives already in existence, some of which had been taken at considerable distances apart in the same eclipse, It would simply be necessary to get two of those on exactly the same scale, and see if they would combine in the stereoscope.

The President remarked that, even if the corona were not rotating, one would get some displacements from two stations at opposite ends of the line, on account of the Earth's motion in longitude. But he believed Mr. Maunder contemplated having some actual stereoscopic pictures made from the 1900 and 1901 eclipses to see if anything could come out of them.

In response to the President's request, *Dr. W. J. S. Lockyer* gave a description of the spectroheliograph erected at the South Kensington Solar Physics Observatory, illustrated by a number of lantern-slides.

The President said they were all much indebted to Dr. Lockyer for his kindness in showing them his beautiful slides, and telling them about the instrument which had just been got into order at South Kensington.

Mr. Evershed congratulated Dr. Lockyer upon the magnificent results he had achieved, especially in photographing prominences. He had never thought it possible, with 14-min. exposures, to get such good definition. He would like to hear from Dr. Lockyer whether he had obtained any of the results described during the spring months of the year.

Mr. Maunder said that he wished heartily to join the President in his thanks to Dr. Lockyer for the favour he had conferred on the Association by giving them an account of his spectroheliograph work at South Kensington. He wished to ask Dr. Lockyer whether, during the eclipse to which they were looking forward in August next, photographs would be attempted in London of the prominences. It was a matter of some importance for comparison with the photographs which they hoped would be obtained by the observers who went out to view the eclipse, because the prominences were practically the only reference points.

Dr. Lockyer said he was very sorry he had not referred to Mr. Evershed's work, and it was a pity that the pictures he had obtained with his spectroheliograph were not more known. He sympathized with Mr. Evershed's remarks as to work in spring and autumn. In reply to Mr. Maunder's remarks, he said that arrangements had been made at South Kensington to take continual photographs of the Sun on the day of the eclipse as long as they were able to see it in London.

The President, in tendering the thanks of the Association to Dr. Lockyer, said they all felt that this instrument was a notable contribution to the study of solar physics, and it was satisfactory that England was not allowing herself to lag behind other countries in this very powerful method of solar research.

Mr. Maunder made a statement as to the present state of their arrangements for seeing the eclipse.

Mr. T. R. Clougher said he had been in communication with a Member of the Royal Astronomical Society of Canada with regard to an expedition to Labrador which was being arranged by the Canadian Government. The weather prospects were not very encouraging. The best that could be said was that the chances were 50 per cent. The matter of transport presented some difficulty, and observers would have to camp out, but everything would be done to make visitors from England comfortable.

The President said they were all very much obliged to Mr. Clougher for the trouble he had taken in finding out the conditions for observation at Labrador. As to Majorca, he had heard that the Grand Hotel, Palma, was very comfortable and the terms quite moderate.

Mr. Krauss Nield recommended travelling by steamer to Bordeaux, and then by the Sud Express so as to reach Burgos early on the day of the eclipse, returning immediately after the eclipse to Santander or San Sebastian.

Mr. Maunder confirmed Mr. Nield's suggestions as to the Bordeaux route, which would avoid the necessity of staying a night at Burgos.

The President said it would not do to trust to this arrangement unless accommodation could be reserved in the express beforehand, and he was not quite sure if that could be done.

Mr. Krauss Nield said he was informed that it could be done if passengers booked early enough.

Mr. Hardcastle exhibited a modification of the Jovian Orrery shown by the President at a recent Meeting of the Association.

The President said that Mr. Hardcastle had certainly developed the instrument in a more striking form.

Mr. Hardcastle gave a summary of a paper by *Mr. S. Stuart* on "Ancient Observations on the Almagest."

Mr. W. T. Lynn said the accusation made against Ptolemy was a serious one, but it seemed to him that it was made on very insufficient grounds, because if Ptolemy could have brought up the precession so accurately from the time of Eratosthenes to his own, he must have known its true value much more nearly than it is at all likely that he did without further observations, so that the argument really went the other way.

The President said it would certainly be rather striking if a man whose name had stood so eminent in astronomy for centuries as Ptolemy should be proved to be a gigantic fraud. He personally could not express an opinion, because he was not well up in astronomy of Ptolemy's period.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The monthly Meeting of this Society was held on Wednesday evening, April 19, at the Institution of Civil Engineers, Great

George Street, Westminster, *Mr. Richard Bentley, F.S.A.*, President, in the Chair.

Mr. W. H. Dines gave "An Account of the Observations at Crinan in 1904, and Description of a new Meteorograph for use with Kites." These observations, which are carried out under the direction of a joint committee of the Royal Meteorological Society and of the British Association, are made with meteorographs attached to kites, with the object of ascertaining the conditions prevailing in the upper atmosphere. During last summer the kites were flown from the deck of H.M.S. 'Seahorse,' which was placed at the disposal of the Committee by the Admiralty. Mr. Dines designed a new and inexpensive meteorograph, which he now fully described. The weather conditions of last summer were somewhat unusual, there being a decided preponderance of east and south-east winds. Near the summit of Ben Nevis the air was often dry and was on several occasions warmer than the air at the same level at Crinan. As a rule, however, the temperature on Ben Nevis is generally much lower than the temperature in the free air at the same level. On several occasions temperature inversions were observed at levels between 3000 and 7000 feet. A fact previously noticed was again observed, viz. the decrease of strength of easterly winds with elevation.

Dr. H. R. Mill also read a paper on the "Rate of Fall of Rain at Seathwaite." This is a discussion of the records from a Negretti and Zambra self-recording rain-gauge during a period of eighteen months. Seathwaite, which is in Borrowdale, Cumberland, is in almost the wettest spot of the British Isles, the average yearly rainfall being about 137 inches. Dr. Mill's results seem to show that the rainfall at Seathwaite in an average year indicates a tendency to be greater during the hours of darkness than in daylight, that rather less than half the time during which rain is falling it continues without intermission for at least six hours at a time, and that rather more than half the total amount of rain is deposited in such long showers.

The Brightness of Planetary Nebulæ.

That "Planetary Nebulæ" are very faint objects is, perhaps, generally known, but their extremely faint luminosity, or intrinsic brilliancy of surface per unit of area, is not, I think, sufficiently realized. Let us examine a few of these objects, and compute what their luminosity is compared with the luminosity of the Sun's surface.

1. H. IV. 37 (G. C. 6543).—This is the well-known planetary nebula near the pole of the ecliptic. D'Arrest found its apparent dimensions to be 23″ × 18″, and Bird estimated its light to be equal to that of a star of the 10th magnitude. Now, taking the Sun's

stellar magnitude as $-26{\cdot}5$, we have the Sun brighter than the nebula by $36{\cdot}5$ magnitudes, or 398,100,000,000,000 times. Taking the diameter of the nebula as 20″, and the Sun's apparent diameter as 32′, or 1920″, we have

$$\frac{\text{Surface of Sun}}{\text{Surface of Nebula}} = \left(\frac{1920}{20}\right)^2 = 9216.$$

Hence

$$\text{Luminosity of Sun} = \frac{398{,}100{,}000{,}000{,}000}{9216} = 43{,}196{,}720{,}000,$$

or

$$\text{Luminosity of Nebula} = \frac{1}{43{,}196{,}720{,}000}$$

of the Sun's luminosity.

2. *h* 3365.—Between δ Crucis and π Centauri. About 6″ in diameter, and 7th magnitude, evidently a much brighter object than the above. Proceeding as before, we have Sun $33{\cdot}5$ magnitudes, or 25,120,000,000,000 times brighter than the nebula, and

$$\frac{\text{Surface of Sun}}{\text{Surface of Nebula}} = \left(\frac{1920}{6}\right)^2 = 102{,}400.$$

Hence

$$\text{Luminosity of Nebula} = \frac{102{,}400}{25{,}120{,}000{,}000{,}000} = \frac{1}{245{,}312{,}000},$$

or about 176 times greater than that of the nebula near the pole of the ecliptic.

3. Σ 5 (G. C. 6120), $16^h\ 40^m{\cdot}3$, N. 23° 59′ (1900).—Planetary nebula 8″ diameter, 8 mag., d'Arrest. Proceeding as before, I find

$$\text{Luminosity of Nebula} = \frac{1}{1{,}095{,}500{,}000}.$$

4. G. C. 7027, $21^h\ 3^m{\cdot}3$, N. 41° 50′.—Planetary nebula in Cygnus, 4″ diameter, $8{\cdot}5$ mag., Webb. Proceeding as before, I find

$$\text{Luminosity of Nebula} = \frac{1}{434{,}000{,}000}.$$

Hence this nebula is more than twice as bright as Σ 5 (above) and about 100 times more luminous (in proportion to its area) than the nebula near the pole of the ecliptic.

Taking sunlight as 618,000 times moonlight, we see that even the brightest of the nebula considered above has a very small luminosity compared with the luminosity of the Moon's surface.

Thus the brightest of them, No. 2 (*h* 3365) has a luminosity of

$$\frac{618,000}{245,312,000},$$

or only about $\frac{1}{400}$ of the Moon's luminosity.

The above calculations will show the extreme faintness of these objects. They seem to shine with merely a phosphorescent glow. The great nebulæ in Orion and Andromeda seem to have a still smaller luminosity of surface. J. ELLARD GORE.

Castor as a Quadruple System.

A QUADRUPLE *star* Castor cannot properly be called, since two of its components lack the quality of shining; but it forms a quadruplex *system* of a singularly interesting nature. It includes, indeed, as a fifth member a ninth-magnitude star, 73″ distant, first noticed by Christian Mayer, S.J., in 1776, which drifts in company with the bright pair; but its revolutions must be inordinately slow. Bradley separated the two chief stars of Castor in 1719, and his rough note of their relative situation availed to ratify Herschel's subsequent discovery of their circulation, estimated to take place in 342 years. Other computers lengthened the span to a full millennium; yet the stars show a preference for Herschel's period, the latest and most authentic orbit, published by Dr. Doberck in 1904, being traversed in 347 years. They are at present more than 5″ apart, and their brilliancy entitles them to rank as, with the exception of α Centauri, the finest revolving pair in the sky. Their magnitudes are respectively 2·0 and 2·9; both shine with pure white light, marked, nevertheless, by an unmistakable difference of spectral quality; since the larger star is, in this respect, analogous to Vega, its companion (Mr. Newall considers *) to α Cygni. Each, moreover, governs a system of its own; each independently circulates with and round a mass strongly attractive, though sensibly obscure, thus forming a quaternary combination of, so far, unique design.

The "Cygnian," or less lustrous member of the original couple (unhappily designated α^1 Geminorum) was the first to disclose its duplex character. This it did by means of the Pulkowa spectrograms examined by M. Bélopolsky in 1896 †. They evidenced changes in to-and-fro velocity comprised in a period of close upon three days, and implying the description of an orbit about equally eccentric with that of the planet Mercury. Its mean radius, as projected on the plane of sight, measures 1,900,000 miles;

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* *Month. Notices*, vol. lvii. p. 575.

† *Astroph. Journal*, vol. v. p. 1.

its actual dimensions are of course greater, but how much greater remains unknown; while the relative orbit, owing to the invisibility of the companion-body, lies outside the scope of determination by the spectroscopic method. The system is, however, distinguished by a curious and well-ascertained peculiarity *. A rapid progressive movement of the line of apsides causes the elliptic path of the star to pivot completely round once in 2100 days. M. Bélopolsky attributes the disturbance to the flattened shape of the connected globes. A compression of $\frac{1}{7}$ would, he calculates, suffice, in an orbit on the scale of Algol's, to produce the observed effect. The eclipsing system of Y Cygni, and perhaps that of Algol itself, is similarly affected.

The importance of these results has been heightened by Dr. Curtis's complementary discovery regarding α^2 Geminorum †. His discussion, in October 1904, of twenty-five plates taken with the Mills spectrograph disclosed a variation in the radial velocity of the chief component of Castor to the extent of some 26 kilometres. And here, again, the disturbing body is only inferentially known to be present; it leaves no spectrographic impression; the lines of the bright star shift in a period of 9·27 days, but show no tendency to double.

The comparative study of the couples thus strangely linked together ought to prove highly instructive in many ways, and will doubtless stimulate further efforts to dissipate the uncertainty which still surrounds the elements of motion in the majestic visual system they unite to form. On the basis of those arrived at by Dr. Doberck, its "spectroscopic parallax" (so to call it) has been computed by Dr. Curtis. It comes out 0″·05, the orbital velocities in line of sight of α^2 and α^1 Geminorum being respectively $+6$ and -2 kilometres per second. Hence a relative speed of eight kilometres, which, in conjunction with a known period and inclination, gives the linear dimensions of the relative orbit and, virtually, the parallax of the stars pursuing it. True, the value obtained is scarcely more than experimental; yet it probably approaches nearer to the truth than Johnson's result of 0″·2, always known to be precarious, and more safely to be accounted of as null and void. Admitting provisionally the exactitude of Curtis's parallax and of Doberck's orbit, we can readily deduce for the system a total mass 12·7 times that of our Sun. If impartially divided between the four constitutive globes, each should then exercise more than thrice the solar gravitative power. Dr. Curtis's measures, however, indicate considerable inequality in its apportionment. The smaller star, on their showing, is in fact three times more massive than its apparent primary. Or, putting it otherwise, the third-magnitude component of Castor *is* the primary body, although possessed of only half the light of its satellite.

This species of discrepancy is not unusual. The progress of acquaintance with the relative masses of binary stars has brought into notice several analogous cases. Mr. Lewis's examination of the Greenwich meridian records has shown the balance of attractive power and light to be rarely adjusted with any approach to equality. The fifth-magnitude component, for instance, of ξ Ursæ Majoris is one and a half times more massive than the brighter star it attends, the "Capellan" spectrum of which accords with its yellow tinge, while an ashen hue characterizes, according to Flammarion, the secondary member of the pair. Again, ζ Herculis consists of a third-magnitude yellow and a sixth-magnitude bluish star, revolving, Mr. Lewis finds *, in orbits of the same size, their sixteen-fold disproportion in luminosity notwithstanding. They have a period of 35 years, and are approaching the Sun at the rate of 54 miles a second. Their parallax has not been determined. The disparity of conditions in ζ Herculis is pushed to a further extreme in 85 Pegasi. The total mass of this rapid binary somewhat exceeds that of eleven suns like ours; and Prof. Comstock has ascertained it to be divided between the components in the ratio 3 : 5 †. But the apportionment is, so to speak, inverted; the larger share belongs to the lesser star—to a star photometrically inferior, by no less than 5·6 magnitudes, to its bright companion. It is, then, actually dominant over a body which emits 170 times more light. The mass-relations of 70 Ophiuchi are similar, though less strongly accentuated. The stars are respectively of fourth and of sixth magnitudes, yellow and purplish in colour; yet the visually inferior member exercises, according to Dr. Adalbert Prey ‡, a four-fold preponderance in governing power. The spectral differences of the pair have probably not been registered; viewed as a single object, they appear of solar type.

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* *Bull. de l'Acad. Imp. de St. Pétersbourg*, t. viii. p. 133.

† *Lick Bulletin*, No. 70.

Dr. Curtis's researches into the movements of Castor may prove helpful towards fixing the evolutionary standing of the somewhat anomalous class of stars designated by Sir Norman Lockyer as "Cygnian." Their prototype, α Cygni, is remarkable for the sharpness of its lines, most of which belong to the "enhanced" category. Its place in the genealogical tree, however, is a moot point. Sir William and Lady Huggins locate it between Rigel and Vega, while recognizing the difficulty raised, on this view, by the abnormal strength of the calcium K §. Mr. McClean ranged the star with the congeners of Sirius. Dr. Scheiner thought it a helium orb in course of transition to the solar stage along the byway marked by α Persei ‖. Sir Norman Lockyer assigned to it a position near the bottom of his curve of rising temperature ¶, its supposed ultimate goal being the condition exemplified by Rigel. It would thus be developing in an opposite direction to that assumed for it by Scheiner. But if it should be verified that the Cygnian

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* *Observatory*, vol. xxiv. p. 38.

† *Astroph. Journal*, vol. xvii. p. 223.

‡ *Astr. Nach.* No. 3946.

§ 'Atlas of Stellar Spectra,' p. 149.

‖ 'Potsdam Publicationen,' Bd. vii. p. 331.

¶ 'Sun's Place in Nature,' p. 313.

star in Castor is more massive, as it is obviously less lustrous, than its Sirian companion, a strong argument will be afforded for concluding it to be more advanced in growth. This being so, it should be regarded as on the downward grade of what, for convenience, we call "temperature." And the genetic relations between Sirian and Cygnian stars, if established in one instance, should presumably be generalized. Some time must, however, elapse before a final decision can be arrived at. So far, only just enough has been learned about the interior mechanism of the wonderful system in the Twins to lend the keenest interest to its future investigation.

Agnes M. Clerke.

Winter Fireballs of 1905.

Quite an unusually large number of brilliant fireballs were presented in the skies of January and February, and I have summarised the objects of which descriptions reached me in the following table. In about one-half the cases it was found possible to compute the real paths, but several of these must be regarded as somewhat doubtful and liable to amendment should additional observations of an exact character come to hand.

The best instance is probably that of the ξ Ursid of February 11 10^h 31^m, which was accurately observed by Prof. Herschel, who also determined its real path (*English Mechanic*, 1905, March 10, p. 109, No. 2085). His values are:—Radiant 167° + 33°, height 65 to 23 miles, length of path 52 miles, velocity 18 or 19 miles per second.

Nearly all the large meteors recently visible have been directed from radiants in Monoceros, Hydra, Leo, or this region.

The list is published in the hope of eliciting further records from fortunate spectators of any of the objects who may not have already communicated descriptions of them.

Under the head of "Observers," in the table, I have given the observed path in several cases where only one observation appears to have been obtained. In these particular instances duplicate accounts would be very valuable.

	h	m	
Jan. 14 ...	10	16	Mr. Packer says he heard a feeble thunder-like detonation 2½ minutes after the disappearance of the fireball.
27 ...	11	59	Mr. Packer estimated the meteor's light as greater than that of the Full Moon. 1½ minutes afterwards a sound like rolling thunder became audible. This, however, could have had no direct association with the meteor as it was too distant; the same remark applies to the noise attributed to the object seen on Jan. 14.
29 ...	6	20	The meteor in its flight, as seen from Farnborough, crossed the planet Venus.
Feb. 11 ...	11	45	Witnessed by a considerable number of observers, but its apparent path amongst the stars not well recorded.

Large Meteors observed 1905 January 9–March 18.

Date.	G.M.T.	Mag.	Probable Radiant.	Height, beginning.	Height, ending.	Length of path.	Velocity per second.	Observers.
1905.	h m		° °	miles.	miles.	miles.	miles.	
Jan. 9 ...	10 9	♃	224 +55 218 +40	...	...	...	Slow.	J. P. Kenyon, Stockport: 335° +80° to 22° +45°.
14 ...	10 16	>♀	119 + 3	60	29	55	15	D. E. Packer, Birmingham; T. Callard, Carmarthen; and 2 others.
27 ...	5 55	=♀	98 −10 130 +30	...	...	...	...	A. F. Parbury, Haslemere: 163° +46° to 203° +46°.
27 ...	11 59	½☽	118 ± 0	66	33	53	7	D. E. Packer, Birmingham, and 5 others.
29 ...	6 20	4×♀	130 +33	57	25	67	23	J. H. Bridger, Farnborough; A. F. Parbury, Haslemere; and 3 others.
Feb. 2 ...	6 40	Brilliant	120 +14 145 + 8	...	...	...	...	Rev. J. Brown, Brighton: 54° +14° to 30° +9°.
11 ...	10 22	>♀	150 − 7	51	32	46	13	H. Thomson, Walker-on-Tyne, and 3 others.
11 ...	10 31	=♀	166 +33	62	20	54	20	A. S. Herschel, Slough; D. E. Packer, Birmingham; and 3 others.
11 ...	11 45	3×♀	145 −13	63	46	40	12	16 observers.
13 ...	7 59	¼☽	59 +36 320 +77	...	...	...	...	J. H. Elgie, Leeds: 68¾° −3° to 72½° −21°.
17 ...	19 15	Fireball	......	...	...	...	...	J. Parsons, Ditchling. Travelled from the west.
27 ...	9 20	V. bright	......	...	...	...	Slow.	A. E. Randell, Southfields, S.W. Travelled downwards in W., so ↘.
27 ...	10 20	1	167 ± 0	71	47	51	24	W. F. D., Bristol.
28 ...	6 0	½☽	17 −10	...	...	...	V. slow.	G. D. Fitzpatrick, Co. Galway.
28 ...	10 15	>♀	220 +40	73	43	61	...	Rev. J. Brown, Brighton, and 2 others.
28 ...	12 10	>☽	150 −11	69	28	94	14	R. Leetham Jones, Temple, E.C., and 11 others.
Mar. 2 ...	7 30	Brilliant	150 −11	...	...	...	V. slow.	Miss Maud Campbell, Queen's Co.: 70° +2° to 42° +7°.
18 ...	9 10	>☽	168 −23	47	43	60	15	Rev. W. F. A. Ellison, Enniscorthy; William Hilliard, Tralee.

	h	m	
Feb. 17 ...	19	15	Visible a few minutes after sunrise, and only one imperfect description available.
28 ...	6	0	Mr. Fitzpatrick heard a sound like distant thunder about 6 minutes after the meteor had vanished.
28 ...	10	15	The meteor divided into two at disappearance. The radiant and heights of the object are doubtful.
28 ...	12	10	Two of the obsevers thought the meteor, at the instant of its maximum outburst, decidedly brighter than the Full Moon.
Mar. 2 ...	7	30	Duration of flight 4 seconds; the meteor left a comet-like train.
18 ...	9	10	The last meteor on the list was a pear-shaped mass of vivid green and white light, 7′ across at its widest part. The meteor swelled out and exploded three times, each time giving a vivid flash like lightning. Duration of flight 4 seconds.

Bishopston, Bristol,
1905, March 18.

W. F. Denning.

A Wet Week in March.

Anyone studying the meteorological part of the Registrar-General's weekly Report could hardly fail to be struck by the unusually heavy rainfall in the week ending 1905 March 18, during which, appropriately enough, the Royal Meteorological Society held their Exhibition at the Institute of Civil Engineers. A paper read by Dr. Shaw to the Statistical Society at about the same time emphasized the value of the "week" as a weather unit, and it was at once suggested that a search through the aforesaid weekly reports would prove of interest. It is now rather more than 60 years since the commencement of the weekly meteorological report from Greenwich Observatory, and during the whole period only 24 weeks have shown a higher rainfall than the 2·25 inches recorded in the wet week last March, including which there have been 50 weeks with totals exceeding 2 inches.

A brief analysis shows that this year has produced the solitary example in March, none having occurred in February, and only one in April (in the year 1878). In fact, dividing the year into equal parts at May 20 and November 20, we find only 8 of the 50 in the winter half; at the same time the summer has no monopoly, for though 11 weeks wholly, and one partially fall in July, 10 wholly and 4 partially fall in October, and of the 42 in the half-year from May 20 to November 20, 22 come before and 20 after the 20th of August. The 28th and 43rd weeks in the year appear in the list 4 times each (the 28th having also the distinction of being the hottest week on the average), while in close attendance come the 29th and 30th, and the 42nd and 44th with 3 appearances each.

Again, of the 50 weeks in the list, no less than 6 owe their inclusion to a single day's fall of over 2 inches—one of these, curiously enough, being the solitary April week in the list, one in October, and the others practically in July. The exact figures for these six wet days are:—

	inches.
1903 July 23	2·47
1888 July 30	2·49
1878 April 11	2·51
1857 October 22	2·57
1853 July 14	2·63
1867 July 27	3·67

The wet summer of 1903 is responsible for 4 of the 50 weeks, 1852, 1865, 1879, 1888, and 1894 for 3 each, except that 1879 cannot claim the whole of the week ending January 4.

Of the 25 weekly totals of 2¼ inches and over, 17 exceeded 2½ inches, 9 were over 3 inches, 3 over 3½ inches, and one, ending 1888 August 4, amounted to 4·29 inches, so that it was quite wet, even excluding the July 30 from the above table. W. W. B.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Richard Dunthorne.

Gentlemen,—

The name of the above is known as that of one of those (not few in number) who, unprovided with early advantages, have succeeded, under the stimulus of love for science, in not only acquiring knowledge, but also making some useful contributions to the progress of astronomy. In Poggendorff's 'Biographisch-Literarisches Handwörterbuch' he is said to have been a "Geistlicher," or clergyman, but this is a mistake. The son of a gardener at Ramsey, in Huntingdonshire (where he was born in 1711), he attracted, at the free grammar-school there, the notice of Prof. Long; and after undertaking for a time the management of a preparatory school at Coggeshall, in Essex, Long, then Master of Pembroke Hall, Cambridge, procured him the appointment of butler to that foundation, and employed him as an assistant in his astronomical work. He also acted as superintendent of the works of the Bedford Level Corporation and on a survey of the fens in Cambridgeshire; the locks on the Cam were constructed under his direction, and he made observations for a map of the county, besides taking part, towards the end of his life, in the comparisons for the *Nautical Almanac.* He died in 1775, only five years after his patron, Prof. Long.

Dunthorne's first scientific publications were 'The Practical Astronomy of the Moon,' dedicated to Long, in 1739, and a 'Letter concerning the Moon's Motion,' giving the results of comparisons between the places from the tables in that work, which was read before the Royal Society in 1747. Two years afterwards another, on the acceleration of the Moon, examined and confirmed Halley's discovery of that fact, giving a numerical determination of its amount. His letter concerning comets was read to the Royal Society in 1751. It gave the approximate elements of the comet of 1264, which appeared so similar to those computed by Halley for the comet of 1556 that Dunthorne concluded that it was periodic, with a period of about 292 years, and would return in 1848. The late Dr. Hind, as is well known, from some investigations which he had made, took up this idea somewhat enthusiastically, suggesting, however, that the comet's return might be delayed by a few years. It has, however, not returned, and there may be some errors in the observations of 1264, on which Dunthorne relied. In the same letter he pointed out the improbability of Halley's conjecture that the comet of 1106 was identical with Newton's comet of 1680. This conjecture was united with that of several previous identifications founded on the theory of a period of about 575 years. Gibbon devoted a long passage in his 'Decline and Fall' to these supposed successive returns, *à propos* of one in the reign of Justinian, which was, in fact, probably a return of the comet of 1682. (Perhaps I may here be allowed to point out a mistake of Mr. Glazebrook in his excellent account of Newton in the 'Dictionary of National Biography,' where he (p. 384, col. ii.) calls the comet of 1680, instead of that of 1682, "Halley's Comet.") Gibbon remarks in a note (Bury's edition, vol. iv. p. 433), "Astronomers may study Newton and Halley: I draw my humble science from the article Comète in the French Encyclopédie." Prof. Bury says in an appendix that "The identity of the comet of 1680 with the comets of A.D. 1106, A.D. 531, B.C. 44, &c., is merely an ingenious speculation of Halley." And the French writer calls it only a conjecture, though he was apparently not aware that Dunthorne had shown the improbability of the conjecture.

Dunthorne computed elements of the orbits of Jupiter's satellites, and had intended to form tables of the motions of all on the model of those of Pound for the first; but want of sufficient leisure from his other occupations prevented his carrying out this design. In his paper on the subject, printed in the 'Philosophical Transactions' for 1761, he is the first, I believe, to use the expression "apojove." J. S. Bailly, in his 'Essai sur la Théorie des Satellites de Jupiter,' published at Paris in 1766, first, so far as I am aware, used the correlative expression *perijove.*

Dunthorne's last astronomical achievement was with regard to the transit of Venus in 1769, which he observed at Cambridge.

Yours faithfully,
W. T. Lynn.

Blackheath. 1905. Mar. 10.

The Comets of A.D. 1264 *and* 1556.

GENTLEMEN,—

In my letter to you last month on Richard Dunthorne*, I alluded to his theory respecting the identity of the comets of 1264 and 1556, and Hind's almost enthusiastic acceptance of that theory, leading to the belief that it would return about three hundred years after the latter date, which it was not kind enough to do. Hind, relying principally upon the calculations of Bastiaan Bomme, of Middelburg (who died in 1858, aged about 55), thought that the return would be delayed until 1858 or perhaps even to 1860. But of course any such view could only be founded upon the supposed identity of the comets of 1264 and 1556. Halley computed the elements of the latter, and included them in his 'Synopsis of Cometary Orbits,' published exactly two hundred years ago (1705). Dunthorne, however, was the first to compute elements of the comet of 1264, founded principally upon places in a paper by Egidius (Giles), which he had found in manuscript in the University Library at Cambridge. These were sufficiently near to those computed by Halley for the comet of 1556 to lead him to suggest their identity, and that the comet, having a period of 292 years, would return again about 1848. Pingre afterwards took up the question. He showed, however, that there were inconsistencies and probably other inaccuracies in the observations of Egidius; but, making use of the Chinese and other records, still thought it probable that the comets of 1264 and 1556 were identical. To use his own expression, although he could not claim great accuracy for the elements of the comet of 1264, he thought they might be as accurate as those of the comet of 1556. It is obvious, however, that if *both* orbits were doubtful, that rather increases than diminishes the doubt respecting the identity of the comets. Hoek, of Leyden, published a paper on the subject in *Ast. Nach.* No. 1060 (vol. xlv. col. 49), in which he determined the elements of the comet of 1264, finding them, on different suppositions of the interpretation of the Chinese observations, so unlike those of the comet of 1556 that the identity did not seem probable.

Valz, of Marseilles, also tried several interpretations of the observations of the comet of 1264, and came to the conclusion: "Il résulterait de la diversité de ces éléments que si l'identité des comètes de 1264 et 1556 est possible, elle n'est pas du moins bien certaine" (*Ast. Nach.* No. 1068, vol. xlv. col. 183). Hoek wrote again in No. 1078, maintaining his own view on some points, but the general conclusion would certainly seem to be similar to that expressed by Valz. Hind, however, would still stick to his view of the identity of the two comets. In *Ast. Nach.* No. 1067, referring to the earlier paper of Hoek, he expresses his dissent from the doubt thrown upon this, and states his intention of shortly sending a communication on the subject, which he never appears to have carried out. But in his book on 'The Comet of 1556,' published in the same year (1857) as these papers, he rejects (p. 5, note) the criticisms of both Hoek and Valz, and expresses confidence that the comet would return by or before 1860. Even 1858 would have been ten years after the time which was due had the period remained as resulted from two appearances in 1264 and 1556, and the extension rather put a strain upon the acceptance of the accuracy of Bomme's calculations. The latest date suggested is now forty-five years ago, and of course all idea of the identity of the comets has long since been abandoned. The moral is that calculations of orbits founded on ancient or mediæval descriptions of comets before the days of accurate observation (which, in fact, commenced with Tycho Brahé) can only result in more or less probable conjectures. Some time ago I pointed out how judicious Halley was in taking that view respecting his own suggested identity of the comets of 1532 and 1661, the path indicated by the descriptions in the earlier of those years being subject to a good deal of uncertainty. Maskelyne, in later times, expected an appearance in 1789, but none such took place.

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* [Published in this number, p. 215.—EDS.]

Yours faithfully,

Blackheath, 1905, April 3. W. T. Lynn.

OBSERVATORIES.

Yale.—A small pamphlet has just been received containing the individual reports for the last five years of Dr. Elkin, the Director of this Observatory. The reports are brief because the excellent work done at this Observatory, being all of one kind, requires little description. The heliometer of this Observatory is devoted to determining stellar parallax, and also to a subsidiary research—the Triangulation of the Pleiades. The final results and discussion of the series of measures on the parallaxes of the ten stars of the first magnitude, issued in 1903 as Part vi. of the 'Transactions,' have already been noticed in these pages. Since that work was done, a research on the parallaxes of stars of large proper motion has been in hand. It seemed to Dr. Elkin that the criterion of propinquity given by brilliance alone was too precarious to warrant much dependence being placed thereon. Practically all the stars in the northern hemisphere with known annual motions equalling a second of arc have been examined. A special determination of the parallax of Arcturus, arranged so as to eliminate any possible effect of its reddish colour, has been carried out. This Observatory possesses an apparatus for photographing meteors, which has been used with modified success.

We regret to see that Dr. Elkin's health is failing, and that he is no longer able to take part in observing. All observations are made by Dr. Chase or Mr. Smith.

Melbourne.—Reports of the Board of Visitors of this Observatory for the years March 31, 1903, and March 31, 1904, with which are incorporated the reports of Mr. Baracchi, the Director of the Observatory, for the same periods, are lately to hand. The Board of Visitors are continuing to urge the Colonial Government to spend more money on the Observatory. The post of Chief Assistant is still vacant, and the Board explain that the filling of this is quite necessary; there is a large accumulation of observations of various kinds which should be printed, and the sanction of the Treasurer is asked for this; thirdly, they ask that money should be spent on structural repairs. There are signs that the Treasury are beginning to realize that something must be done, for they have granted £700 for the building of a new room to be used as a laboratory for standardizing weights and measures and for testing air-meters—a public work which has lately been transferred to the Observatory from the Customs Department: a somewhat extraordinary utilitarian view to take of the functions of an astronomer.

There has been a suggestion of federalizing the Australian observatories, and on this point the Board of Visitors recommended to the Chief Secretary of the Commonwealth that all meteorological work should be placed under a Federal Bureau, which should be controlled by a meteorologist of high standing, but that the Melbourne Observatory should continue to be independent. Also, in connection with the proceedings at the Colonial Conference held in London in 1902, Mr. Baracchi presented a memorandum pointing out the advisability of introducing the metric system of weights and measures in the Colony.

The principal astronomical work of the Observatory is in connection with the Astrographic Catalogue and Chart. The observations with the 8-inch transit-circle, of which approximately 2000 have been made in each year, have been chiefly of stars to be used as reference stars for reduction of the Chart plates; the remaining stars are from Sir David Gill's list of zodiacal stars. Of Astrographic plates the total record at date of the Second Report, 1904 March 31, was:—

Catalogue Series, 1149 plates, complete.
Second Catalogue Series, 403 plates taken.
Chart Series, with centres at even degrees of declination—exposure of 60 minutes: 565 plates taken, *i. e.* complete.
Chart Series, with centres at odd degrees of declination—triple exposures of 30 minutes each: 402 plates taken.

The measurement of these plates taken at Sydney and Melbourne is being carried on at a Bureau maintained at the joint expense of the Colonial Governments. The total numbers of complete measures are:—239 Sydney plates, containing 137,812 stars, and 522 Melbourne plates, containing 151,343 stars.

The photographic registration of the magnetic elements and the absolute observations have been carried on as usual. The reduction of the observations of this kind for the years since 1868 is proceeding.

PUBLICATIONS.

Analytical Dynamics *.—In this work the author treats dynamics as essentially a branch of pure mathematics: the equations of motion once obtained, their further treatment is a matter of analysis. The simple and soluble problems are treated in the same way as the more complex ones, by means of generalized equations, and all such methods as resolving in special directions or taking moments round particular points are avoided. The treatment of these questions serves as an introduction to the view of theoretical dynamics which is developed in the latter part of the book. After a preliminary chapter on kinematics, the equations of motion are found. Here the author follows Mach, and instead of the laws of motion in Newton's form, the statement of the experimental laws on which dynamics is based is given in the form: "If any set of mutually connected particles are in motion, the acceleration with which any one particle moves is the resultant of the acceleration with which it would move if perfectly free, and accelerations directed along the lines joining it to the other particles which constrain its motion. Moreover, to the several particles A, B, C, . . ., numbers m_A, m_B, m_C, . . . can be assigned, such that the acceleration along AB due to the influence of B on A is to the acceleration along BA due to the influence of A on B in the ratio $m_B : m_A$. The ratios of these numbers, m_A, m_B, . . ., are invariable physical constants of the particles." The entire omission of Newton's name in this connection is enough to make the late Professor Tait turn in his grave. The equations of motion are obtained in Lagrange's form at the outset, and the solution and transformation of these equations is the subject of the book. The case of ignorable coodinates is next considered, as being the principle most usually available for the integration of the equations, and it is shown how this and the equation of energy are generally adequate for the solution of the simple problems of particle and rigid dynamics. Vibrations about positions of rest or steady motion are considered in detail, the difference between the two cases due to the presence of terms involving products of the coordinates and velocities in the latter being clearly elucidated. A consideration of dissipative systems concludes the discussion of the soluble problems of dynamics.

The remainder of the book deals with the higher and more general problems of theoretical dynamics. The principles of least action, Hamilton's transformation of the equations of motion, the equation of Hamilton-Jacobi are given with modern

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* By E. T. Whittaker, M.A. Cambridge University Press. Price 12*s*. 6*d*.

developments and extensions. Particular stress is given to the application of Lie's contact-transformations to dynamics, a general theory which the author states to underlie the solution of all dynamical systems.

The last chapters of the book are devoted to the problem of three bodies and the general theory of Orbits. The author tells us that between 1750 and 1904 over 800 memoirs, many of them bearing the names of the greatest mathematicians, have been published on the problem of three bodies. Mr. Whittaker's Report on the problem of three bodies to the British Association in 1899 marks him as having very special qualifications for dealing with this subject.

The theorems of Bruns and Poincaré on the impossibility of finding other than the known classical integrals of the problem are fully considered. It should be added that much of the subject-matter has been hitherto accessible in scattered memoirs by various investigators, particularly Lie and Poincaré. F. W. D.

POPULAR STAR-MAPS *.—Plans and diagrams for teaching the constellations may be found in many books, but we do not remember a series of maps for the purpose on a scale so large as the one now before us. A fault of the ordinary star-map found by the beginner is that it attempts too much—the small stars are given in such detail that they confuse, and one "cannot see the wood for the trees." This and other objections the compiler of these maps has sought to obviate by indicating only the stars down to the 3rd magnitude, or in exceptional cases to the 4th, and by using a blue ground, so that each chart looks like the sky on a not especially brilliant night. There are ten maps in all—two polar maps and four having the equator and the meridian 0^h, 6^h, 12^h, or 18^h respectively as axial lines, each extending over 6 hours of right ascension, so that these six maps cover the sky; and then there are in addition four similar maps with the meridians 3^h, 9^h, 15^h, or 21^h as the central line, but it is necessary to qualify this by saying that no meridians or circles of declination are marked on the maps, nor even on the key maps, and only on these latter are the names of the stars given, so that the charts themselves show stars only. There is a small introduction explaining the method of projection and a list of the names of the constellations and all the stars shown in the maps. The whole is contained in a neat blue cloth case, and we feel that Comte de Miremont may be congratulated on his production.

THE REGISTERING TRANSIT-MICROMETER.—It is not necessary to say anything by way of description of this instrument, for it has by this time taken its place among instruments of precision,

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* 'A Rapid and Easy Method of Finding the Principal Stars.' By Comte de Miremont, F.R.A.S. London: George Philip & Son. Price 10s. 6*d.*

and a recent publication * of the U.S. Coast Survey is interesting since it gives an account of experiments made to find out exactly how precise it is, and how far it is superior to that of the older method of transit observing.

Passing over all details of the experiments, except saying that the micrometer heads were turned by hand and not by motive power, the main results may be given as follows :—

(A.) The probable error of a time-determination deduced from the observation of one star made by an experienced observer with the registering micrometer is 50 per cent. greater than it would be for a similar determination made by an expert using the tapping method. For an inexperienced observer the probable error would be 100 per cent. greater. This refers to accidental errors only.

(B.) For J. F. H., an experienced observer, the error of a time-determination was much greater for slow-moving stars than for equatorial stars. The excess was much greater than it would be for an expert with the tapping-key. On the other hand, in the case of all the other observers using the registering micrometer, the accuracy is more nearly the same for stars of all declinations than it would be if the tapping-key were used.

(C.) The relative personal equation between two observers with a registering-micrometer is so small as to be masked by the accidental errors of observation, and is in every case probably less than $0^s{\cdot}050$.

The observations with tapping-piece with which the results of these experiments were compared were the observations made in the primary longitude-determinations of the U.S. Survey. In explanation of the use of the word "experienced," it should be said that the observers who took part in the experiments, 16 in number, were divided into classes according to the amount of acquaintance they each had with astronomical instruments. Mr. J. F. Hayford, who writes the account, had already had some experience with the registering micrometer, which none of the others had, so that he stands in a class by himself. It may also be added that in the course of the experiments it was noticed, as might have been expected, that the gearing of the driving-heads had some effect on the results, and it was found that a speed of 4·8 seconds per revolution for the driving-heads of the transit-micrometer for an equatorial star is most favourable for accuracy, but that the change from a much greater speed to this did not cause a very great increase of accuracy.

Now it might be thought from the paragraph (A) above that, since the probable error of a time-determination is so much greater, the registering micrometer is not a particularly suitable instrument for longitude or other work, which is contrary to the experience of certain German officers who have determined several longitude-arcs by its aid; but it is explained that though the

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* "A Test of a Transit-Micrometer," U.S. Coast and Geodetic Survey Report, 1904, Appendix No. 8.

accidental error of observation made with a tapping-key may be small, the results of longitude-determinations made by this method show that there remain in the mean result for a night, and even in the mean result for a pair of stations, constant errors, which are apparently independent of, and much larger than, the accidental errors, a fact which is well known. It is then *supposed* that these constant errors are due mainly to variations in relative personal equation, and it is argued that as the personal equation is eliminated these constant errors disappear, and the final result will be better by the new method than by the old.

Numerical illustration taken from the work of the U.S. Survey is given as follows :—

Prob. error in one night's result arising from accidental error	$\pm 0^{\mathrm{s}}{\cdot}013$
The error peculiar to each night	$\pm 0{\cdot}022$

and the constant error peculiar to each longitude-determination and not capable of elimination by increasing the number of nights is also $\pm 0^{\mathrm{s}}{\cdot}022$. From this it appears that the principal errors are those of a constant kind, and that the effect of the accidental errors is insignificant. Mr. Hayford then goes on to imply that when observations are made with a registering micrometer the constant errors may be neglected, or rather that they do not exist, and computing on this assumption, solely from the data of the accidental error, it is found that a determination of a difference of longitude from a single night's observation with a registering micrometer would be equivalent in accuracy to a ten nights' determination made with a tapping-key.

This reads very satisfactorily, but it must not be overlooked that the conclusion of the argument depends on the premisses that the systematic errors of a longitude-determination are wholly those of personal equation, and that these do not occur when the transits are registered automatically—axioms which are a little doubtful. Certainly the observers of the Prussian Geodetic Institute have got some excellent results with microscopic probable errors, but at the moment they have not completed circuit, so that as yet they have no means of knowing if their work does contain systematic or constant errors. It will be remembered that the German instruments are of the *coudé* type, and the micrometer is placed at one end of the axis. The American micrometers were attached to small transits of the usual form, and the difference of class of instrument may make a difference in the result. H. P. H.

THE SCIENCE YEAR-BOOK, 1905 *.—It may seem a little late to review a publication of the diary class, but on consideration it will be realized that a better opinion can be formed of the utility of such a work after a month or two's use than at first inspection. This year-book, issued in conjunction with our contemporary *Knowledge*, now edited by Major B. F. S. Baden-Powell, has several points to recommend it as a diary. Every day of the year has a page of nearly blank paper, headed with times of sunset, sunrise, the constellations on the meridian at 9 P.M., the records of temperature as taken from the Greenwich 50-years' record, with spaces for the insertion of observed meteorological facts of the actual day. There is a great deal of useful astronomical and other scientific information in the book, which we have not space to particularize.

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* *Knowledge* and *Scientific News*, 27 Chancery Lane. Reduced price 2*s.* 6*d.*

NOTES.

Comet Notes.—M. Gautier's search ephemeris of Tempel's First Periodic Comet is as follows:—

	R.A.			S. Dec.			R.A.			S. Dec.	
1905.	h	m	s	°	′		h	m	s	°	′
May 6....	17	41	13	21	43	June 7....	17	26	22	25	44
14....	17	40	8	22	40	15....	17	19	59	26	42
22....	17	37	4	23	41	23....	17	13	54	27	34
30....	17	32	18	24	43	July 1....	17	8	48	28	19

The following elements of Comet 1905 *a* (Giacobini) are by E. Strömgren from observations on March 26, 28, 30:—

T........	1905 April 3·21 Berlin M.T.
ω........	$357^\circ\ 9'$
☊	156 8
i........	41 37
log q	0·0523

The comet was of the 12th magnitude at discovery, and the brightness is steadily diminishing. It is therefore of little interest.

Approximate ephemeris for Paris Midnight.

	R.A.			N. Dec.	
	h	m	s	°	′
May 4	8	57	21	46	57
8	9	21	48	47	56
12	9	46	17	48	12

A. C. D. C.

Minor Planet Notes.—A new planet, QG, of magnitude 13·5, was discovered by Dr. Wolf at Heidelberg on March 26.

A. C. D. C.

Obituary.—With much regret we have to announce the death of Otto Struve, Director of the Poulkowa Observatory from 1862–1890, which took place on April 14 last. A notice of the life and work of this famous astronomer will be given in our next number.

Announcement is also made of the death of **Lord Grimthorpe**, who, when Sir Edmund Beckett, was a comparatively frequent attendant at the meetings of the Royal Astronomical Society. When it was proposed in 1881 to establish the Solar Physics Committee at South Kensington, he was a leading opponent of the scheme who formed the Society for Opposing the Endowment of Research. His book 'Astronomy without Mathematics' is well-known, and also another on 'Clocks, Watches, and Bells.' He was a recognized authority on the latter subject, and, in conjunction with Sir George Airy and Mr. Dent, designed the clock "Big Ben" for the Houses of Parliament in 1851. Born on May 12, 1816, he was the son of Sir Edmund Beckett, Bart. He was educated at Eton and Trinity College, Cambridge, graduating as thirtieth wrangler in 1838. He practised at the Parliamentary Bar, and was raised to the peerage in 1886.

Recent Earthquakes.—The past month has been remarkable for several earthquake-shocks. On April 4, shortly before 6 o'clock in the morning, a large part of North-western India was severely shaken by an earthquake, which did considerable damage to property and caused much loss of life. On April 23, about 1.30 A.M., an earthquake-shock was felt in England over a large area, including parts of Yorkshire, Derbyshire, Nottinghamshire, and as far south as Berkshire, and between 2 and 3 in the morning of April 29 Switzerland was visited by an earthquake. To these may be added a seismic disturbance of some magnitude experienced at Perth, West Australia, news of which has come to hand during the month. It seems worth while to recall the fact that Prof. Milne has pointed out that earthquakes may have some connection with the variation of latitude, for they are more frequent when the pole is moving quickest or when it changes its direction of motion most abruptly. Judging from the observations already published, the pole is now describing the outermost part of the spiral curve, and is therefore moving fast.

The Discovery of Neptune.—Notwithstanding the editorial postscript to Mr. Ellis's letter in last month's number to the effect that no more contributions on this subject were required, it seems fair to publish the following facts about Prof. Challis which have been communicated to us by one who knew him well. Prof. Turner in his book writes of Challis as being shy and diffident of his own powers, but our correspondent says he was anything but that, although he was very slow and hyper-methodical. As to his asking Airy to lecture *for* him, it is stated on p. 104 of Airy's Autobiography that, before he left Cambridge, Airy *offered* to give the next course of lectures after he came to Greenwich. We are also informed that there is still another man besides Galle alive who took part in the discovery of Neptune—J. H. Morgan, who helped Challis at Cambridge. He was afterwards Chaplain of Bethlehem Hospital for the Insane, and is now an inmate there.

SIR ROBERT BALL.—This is *not* an obituary notice, but Sir Robert has been brought very much to everyone's mind lately, for he formed the subject of the Cartoon in *Vanity Fair* of April 13, and his genial smile has been filling the railway-bookstalls, so we take leave to reproduce "Jehu Junior's" short biography:—

> Sir Robert Stawell Ball, Lowndean Professor of Geometry and Astronomy at Cambridge, applies a merry eye to the telescope, and smiles benevolently upon the stars. The combination of wit with the higher mathematics and of a warm heart with the unimaginable frigidity of space, is suggestive of Ireland to the intelligent biographer. Such characters are born, not made; and Sir Robert was born in Dublin. English schools and Universities have taught him to tolerate the Sassenach; but they have failed to eradicate that most precious of all possessions, his Celtic sense of humour.
>
> Sir Robert comes of a learned family. His father was a naturalist of reputation, one brother became Director of Science and Art at the Dublin Museum, and another, Sir Charles Ball, a distinguished surgeon in the same city.
>
> His mathematics brought him high prizes at school and college, and subsequently procured him a post in Lord Rosse's observatory at Parsonstown, King's County. From that date he has mounted the astronomical ladder two steps at a time. A mathematical Professorship at Dublin was followed by his appointment as Astronomer Royal for Ireland. His lecturing tours at home and in America were planned at the Observatory, near Dunsink. It was there that he wrote the majority of those books on astronomy that have given simplicity and popularity to a recondite science. As these virtues of style are apt to be regarded with suspicion and indignation by more pedantic mathematicians, Sir Robert saved his good name amongst the professors by a *magnum opus* on the 'Theory of Screws,' which, though profound, has nothing to do with Mr. Chamberlain. In 1892 he left the Irish Observatory to sit in the chair that Cambridge had offered him.
>
> Sir Robert prefers the music of the spheres to Wagner; he has made himself plain on that point. He does not ride to hounds, and yet is a judge of horseflesh. He extends his good temper to a golf ball even in its most evasive moods. Recently, he has developed a taste for politics, but ambition has not driven him into candidature, though he is the Chairman of the Cambridge Conservative Caucus. He is a Commissioner of Irish Lights and Lighthouses. Doubtless, it was to aid him in his labours that the Spectacle-makers made him a liveryman of their body.
>
> A wise, witty, and brilliant Irishman, of wide reading and worldly experience—such is Sir Robert Ball.

THE PARTIAL LUNAR ECLIPSE OF FEBRUARY 19.—The Rev. S. J. Johnson sends the following peculiar feature of this eclipse that he noticed:—When one-fourth of the Moon was eclipsed the umbra was of a brownish tint; when one-third was covered a still brighter tint suffused the shadow; but when the five digits were attained at greatest obscuration, and up to the end, the umbra became dense and no lunar details were visible.

A Tenth Satellite of Saturn.—Just as we go to press, news comes of the discovery by Prof. W. H. Pickering of another satellite to Saturn, having period of 21 days, which is about the same as that of Hyperion. It may be remembered that the suggestion has been made that Hyperion is one of a group of small satellites analogous to the small planets of the solar system. The newly-discovered object is said to be three magnitudes fainter than Hyperion.

Correction.—Mr. Denning tells us that in his letter on the "Great Red Spot on Jupiter" in last month's number, p. 188, the remark that the motion conformed with the rate of System II. since August 1904 should have read "since October 1904."

Visitation-Day at the Royal Observatory, Greenwich, will be on June 3. There will be no Visitation Dinner after the Meeting.

There will be an Evening Meeting of the Royal Astronomical Society on May 3 at 8.30 p.m. The next Ordinary Meeting will be on Friday, May 12. The next Meeting of the British Astronomical Association will be on Wednesday, May 31.

From an Oxford Note-Book.

It is pleasant to see 'Bulletin No. I.' of the Kodaikanal Observatory. The difficulties and delays in starting the Observatory have been serious; but at last a start has been made, and it is to be earnestly hoped that it will be followed up. The Director has as yet no European Assistant, but there is at least a possibility that this deficiency will soon be filled. Meanwhile he is working under difficulties, but he has shown already that such conditions do not prevent, though they may retard, progress, and we shall hope for 'Bulletin No. II.' before very long. It is greatly to the credit of Mr. C. P. Butler that the first observations made at the new observatory and contained in 'Bulletin No. I.' were made by him, or under his direction, during the absence of Mr. Michie Smith on leave.

There are wide differences of opinion (as I found during my visit to America) about the usefulness of Bulletins or Circulars as a form of publication. Librarians find them troublesome, of course, and their circulation being more limited than that of astronomical journals of the usual type, many of the readers of the latter have cause to complain that they do not get interesting information. I cannot but feel that these difficulties may be got over by keeping steadily in view one definite principle, viz. that there is little or no objection to duplicate publication if it does not mean duplicate expense. Indeed there are positive advantages in it, as has practically been recognized. What is the striking off of separate copies of a paper for distribution but "duplicate publication" in one form? We recognize the advantages in this case, and are content to overlook the trifling and obvious disadvantages. It seems to me that the bulletins and circulars can be regarded in the same way if only sufficient copies are struck off, not only for circulation at the time, but for incorporation in some permanent journal or publication. Thus one method would be to strike off (say) 700 copies of each circular; to distribute 300 of them at once; and when 50 circulars had been issued to bind the other 300 as a volume for distribution. The librarians would then be spared the trouble of looking after the individual numbers, which could be treated as daily papers are treated—glanced at and thrown away if not needed. Another way would be this: adopt for the circulars the page and type of some standard journal, strike off copies for immediate circulation, but also at the same time enough copies to appear in the journal in due course. Better suggestions will probably be made, but the bulletins are so obviously useful in many ways that I hope they may be improved in some way so as to no longer annoy the librarians—a worthy class of astronomers who merit the utmost consideration.

A PARAGRAPH in the Report for 1903–4 of the Board of Scientific Advice for India has an unusual interest for astronomers. Some years ago Major Burrard, R.E., called attention to the fact that what had been called "local attractions" seemed to follow a general law, and that the facts could be represented by supposing the existence of a subterranean chain of attracting matter running parallel with the Himalayas and to the south of them (see Mon. Not. lxii. p. 180). The evidence available was, however, scanty—not enough to establish the hypothesis, but sufficient to suggest further investigation. The paragraph given below shows that these investigations must have given considerable satisfaction, and it is to be hoped that we may soon hear more of the details:—

> VII. **Geodetic.**—1. ASTRONOMICAL LATITUDES.—The observations made in 1901 with the object of testing the correctness of Major Burrard's theory that the deflections of gravity in India—hitherto attributed to accidental and local attractions—could be classified by regions, were continued in 1902–1903. The result of those observations is that it can now be prophesied with tolerable certainty that on all Himalayan meridians the direction of gravity will be found to follow one general law. In the neighbourhood of the tropic as we move northwards its direction will change from northerly to southerly; it will then remain deflected towards the south for some hundreds of miles, and it will again become northerly as the Himalayas come into view. The substitution of this original law for the old theory of local attraction will probably exercise a profound influence on future investigations and programmes.

THE latest news from Mount Wilson is distinctly good. Experiments with the Snow telescope are very promising, the range of focus of the 60-foot mirror being now less than 3 inches during the day. But there has been delay in getting the prisms for the spectroheliograph, and the weather in the early part of the year was bad. This was not unexpected, being in accord with the experience of the early months of last year; but dark rumours have reached us of a specially evil influence in this particular year, in the shape of a man who has a secret process for making rain by ejecting some chemical compound into the air, and had a contract with the city of Los Angeles to make 18 inches of rain before April 1, for which he was to receive $1000. Whether he earned his money we have not yet heard; but astronomers are only human, and however much they may theoretically laugh at rain-makers, when a beautiful night suddenly comes cloudy just as telescopes are set for work, it is difficult to resist the suspicion that the rain-maker has been "squirting some of his medicine into the air." However, his contract has now expired one way or another, and conditions are improving. We hear of beautiful photographs being taken, and of a comet being discovered by photography, which, however, turned out to have been found independently a week before, though intelligence of it had not reached the lonely mountain.

TALKING of rain-makers, there is an interesting little story in *Symons's Meteorological Magazine* for November last which I venture to commend to others, especially since it opens with a very flattering reference to some of these Notes. In November last a railway-journey was described which was much interfered with by heavy rains. Dr. Mill travelled the same route a month later, but his attention was directed to another part of it where rain had been scarce, and he was fortunate enough to witness the beginning of a sacred "rain-dance" (lasting for three days, but he did not see it out) which was to relieve the situation. The rain came down in torrents, but whether in response to the entreaties of the dancers or as a divine vengeance on another tribe of Indians who sold their rain-god to one of Dr. Mill's fellow-travellers (not with Dr. Mill's approval—he was distressed at the transaction) cannot be determined. But the story should certainly be read in full.

LETTERS for those on Mount Wilson should now be addressed "Observatory Office, Pasadena." Some time ago I sent Prof. Hale an extract from some Racing News showing how "*Pasadena* wins the *Sunshine* Stakes," and he replied with a label from "Thos. *Glass* and Sons, Agents for Browning's *Solar* Water Heater, *Corona*, California."

IT is curious how the word "telescopic" has become associated with the behaviour of the tube (the least essential part of the instrument, as astronomers think) rather than with the optical portions of the instrument. We all know what is meant in a railway accident by one carriage being "telescoped," and no optical figures at all are called up in our minds by the use of the word, even though we be astronomers. Similarly those who have shown novices over an observatory know that a large telescope is often expected to have an elaborate tube,—bigger compartments to pull out, and more of them. Father Cortie sends me the following interesting conversation between two Lancashire trippers overheard near his observatory:—"Eh, lad! what's yon?" "Yon?" said number two: "why its a moon-shop, where they study economy." "Eh! but I can see end of telescope," added his companion. "Can you, lad? But you should just see it when its pulled out! It stretches half over yon field."

Answers to Examination Questions:—

The orbits of the minor planets lie between those of Mars and Jupiter, and are so entangled that it is believed that if one were to be picked up all the others would be lifted at the same time.

Jupiter's Satellites were discovered by accident, by a man who was observing Jupiter.

It is very difficult to follow these satellites (of Jupiter) night by night, as you get so muddled as to which is which, and you can't make out which one has gone; but if you have an almanac you can follow them, and it is a very interesting study.

Sometimes one moon is in transit and another in occident, and they pass one on either side of the planet. The orbit of 4 is much larger than that of 1, but occasionally their transit and occident is identical.

When a moon passes across the planet's face, it casts a strong shadow on the surface of the planet, and it is interesting to notice that sometimes the shadow is in a different direction, according to the position of the Sun.

The following paragraph from the *St. James's Gazette* of 1904 August 4 might have found a place alongside the account of the wonderful meteor recorded last month:—

The 'Victory's' last battle was not fought at Trafalgar. During the last year or two, in her resting-place at Portsmouth, she has warred against fire and air, and once at least narrowly escaped being smashed up by a modern battleship running astray. Yesterday an aerolite, "fast coursing from the sky," missed her almost by a hair's-breadth. Surely some little cherub sitting up aloft keeps a watchful eye on the old ship. Her many narrow escapes should persuade those prosaic men who desire, or did desire some short time back, to make an end of the 'Victory,' that Fate has a finer destiny in store for Nelson's glorious death-bed.

The near passage of a motor-car seems to be more dangerous than that of a meteor, to judge from the following extract (reference mislaid):—

In a Surrey police-court case, a motorist charged with furious driving swore that he was only going at the rate of fifteen miles an hour. Of course one accepts his word. Now we come to the testimony of a road repairer. He swore that when the motor passed him it created such a draught that his heavy road shovel revolved right round him three times. One must also accept the road repairer's sworn statement. However, this scientific conclusion may be drawn from the case: Had a hundred-horse-power Panhard passed at full speed the road repairer would have evidently become a human planet, with a shovel satellite revolving perpetually around him.

The introduction of the motor seems to have had a revivifying effect on the imagination of the country folk.

ALONGSIDE these portents the story of the Cherbourg light should find a place. Perhaps the following two extracts will give it sufficient representation. From the *Daily Telegraph* of April 4:—

> This story from Cherbourg is dated, not April 1, but yesterday. A ball of light rose at eight o'clock in the evening over the sea. Its apparent diameter was about seven inches, and it was surrounded by a glowing halo extending to some fourteen inches from the circumference of the orb. The latter sailed majestically towards the land, passing over the forts commanding the roadstead, over the harbour and the inner basin, then over the town itself, remaining at a considerable height. Then it slowly circled round, and was wafted away in the direction whence it had come. All Cherbourg was out in the streets to watch the phenomenon. Was it a luminous balloon, some mysterious signal, a new kind of slow travelling meteor, or a Martian airship come to prospect the Earth? The ball of light remained visible for three hours and a half. During the time the naval authorities kept marine glasses pointed at the phenomenon, but failed to determine what it was. It is stated that the admiral has reported on the occurrence to the Ministry of Marine in Paris, and meanwhile has ordered a torpedo-boat to be in readiness to pursue the luminous orb out to sea if it should reappear.

From the *Evening Standard* of April 6:—

> MYSTERY OF THE SKY.
>
> STRANGE NIGHT PHENOMENON AT CHERBOURG.
>
> (From Our Own Correspondent.) PARIS, Thursday.
>
> The mysterious light or luminous globe reappeared at Cherbourg last evening *, and was seen from 8.30 till 11.0. Dense crowds assembled in the streets and public squares to witness the phenomenon. The light followed the same course as when seen last week. The naval authorities are now convinced that it has nothing to do with any supposed experiments in the way of signalling by lights from captive balloons.
>
> Those who note coincidences remark that the planet Venus has not been visible since the first appearance of the phenomenon.

THE publicity following on the announcement of the award of the R. A. S. Gold Medal to Professor Boss has led to an even greater honour for him. He has appeared in *Punch*! Among the "Charivaria" for February 22 (p. 134) occurs the following:—

> The Director of the Albany Observatory, New York, bears the title of Principal Boss. It is rumoured that Sir HENRY CAMPBELL-BANNERMAN is seriously considering the adoption of this impressive title, with a view to increasing his authority.

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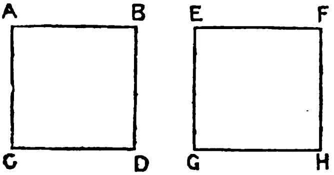
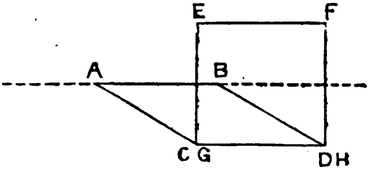
* April 5.

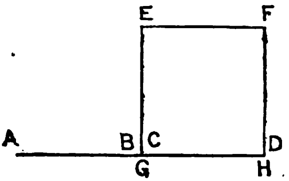
A CORRESPONDENT in Australia kindly sends me a mathematical poem. I fear it is too long to insert as a whole, but the following portions of it contain the essentials :—



Let it be granted (though I have my doubts)
That when aged twenty-five, or thereabouts—
Each public man that walks and breathes the air
Is honest, upright, principled, and square.
So far, so good :—but now we will suppose
A B D C the way of all flesh goes ;
From the straight path of virtue he inclines,
Through sordid and ulterior designs,—



Palpable that of Fortune he's a suitor,
He thinks, no doubt, his ends are much acuter.
And so they are. (While, between me and you, Sir,
His sense of rectitude becomes obtuser.)
One for the other, I am not unfair
In holding that his ends are far from square.
But what I wish to demonstrate is this—
As pointed out in my hypothesis—
That though these men are equal as to " side "
Their inequality can't be denied.



Come, I'll conclude this theorem of mine,
For could he further from E G incline?
Scarcely—yet possibly—but if he tried
The sides B D, G H, might coincide.
And then—the final figure proves the case,—
Then uprightness is gone, and all is " base."

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1905 May 12.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: E. T. WHITTAKER, M.A., and
T. LEWIS.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Lewis. Since our last Meeting the Society has received 69 presents. Of those calling for special reference I may, perhaps, mention two—a map of England and Wales, prepared and presented by the Rev. S. J. Johnson, on which the path of the Moon's shadow during the eclipse of June 1927 is laid down; and we have also received a small book by M. Bigourdan, who is with us to-night, entitled 'Les Eclipses de Soleil.'

The President. We have with us this evening, as you have heard, M. Bigourdan, of the Paris Observatory, and I will ask him to say a few words about what he is going to do at the forthcoming eclipse.

[M. Bigourdan then addressed the Society in French *.]

The President. We are, I am sure, very much obliged to M. Bigourdan for his address, and I am sure we all wish to see him again and to receive verbally from him some account of his work.

Mr. Dyson then read a paper by himself and *Mr. D. J. R. Edney* on "Discussion of the Observations of the Satellite of Neptune made at the Royal Observatory, Greenwich, in the years 1902–3–4." He remarked that specimen photographs had already been shown, so that Fellows were familiar with them. The movements of the satellite of Neptune have been worked out in a beautiful memoir by Dr. Hermann Struve, who put together all the observations to 1890–1–2. The memoir served for the elements which were given each year for this satellite in the 'Connaissance des Temps.' A comparison with the tabular places derived from this source had been made each year with the distance and angles found from measures of the Greenwich photographs, and it was a discussion of these differences which Mr. Edney and himself had made. In each year there were about 50 or 60 photographs, and it was found from a discussion of the residuals that the probable error of each measure of distance or of angle as derived from each plate was $\pm 0''\cdot 14$, which, considering the object, was quite small. The mean distance of the satellite is found to be slightly smaller than that given by M. Struve. The eccentricity is found to be insensible. Values of this quantity previously obtained from visual observations are somewhat discordant, possibly owing to personality in the measures. The movement of the node of the satellite on the Earth's equator, and the change of inclination, pointed out first by Marth, has been explained by Tisserand and Newcomb as caused by the spheroidal figure of Neptune, so that the interesting problem is now presented of obtaining the position of the equator of Neptune, the oblateness of Neptune, the inclination of the satellite's orbit to the plane of Neptune's equator, and the period of the revolution of its node from the changes mentioned above, found from these photographs. A determination of these elements, though at present only a rough one is possible, has been made. It seems clear that a continuation of the observations will be very valuable for a more accurate determination in 10 or 20 years' time.

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* M. Bigourdan will be a member of a French official expedition to observe the eclipse of August next, probably from a station near Sfax, in Tunis.

The President. We must congratulate Mr. Dyson and Mr. Edney upon the completion of a very interesting piece of work, and one which must have permanent value.

Prof. Turner. I was not here at the reading of the early part of the paper, and I am not sure whether Mr. Dyson told us which way the node goes, and whether the change of inclination of the satellite's orbit will make it more inclined or less inclined to the general plane.

Mr. Dyson. I really cannot say at this moment.

Prof. Turner. Perhaps I may say a word or two on some points in a paper "promised," but not yet complete, on the diameter of photographic stellar images, if Fellows will forgive a somewhat inadequate preparation. We have now come to the end of our measures of the Astrographic Catalogue, and we have been considering which approximate formula best connects the measured diameters of stars with their magnitudes found from visual observation. The formula that suggested itself in the first instance for connecting diameter and magnitude was represented simply by a straight line, but in the volume of the Astrographic Catalogue lately issued from Greenwich it appears that a formula of the form $m = a - n\sqrt{d}$ fits the observations. In the early days when we had not very much material to go upon I did not feel sure that our Oxford measures fitted this formula: and it seemed better on the whole not to adopt it, but a recent more complete discussion showed that *when the systematic errors of Argelander's visual scale were corrected*, the Oxford measures were well represented by the Greenwich formula. One or two facts present themselves from consideration of the values of the coefficients given in the Greenwich Catalogue. Different numerical values of a and n are given, and it will be noticed that the values of a and n always increase together: which shows that the lines represented by the several formulæ pass through a point. Hence it follows that though the diameters of faint stars are on certain nights (probably poor nights) smaller than would be expected from the formula, on the same plates the diameters of bright stars are actually *larger*. I think that this can be explained by the fact that the poorness of the night consisted in the greater wandering of the images. In the case of the faint stars the wandering images are too faint to impress themselves on the film at all, in the case of the very bright stars the images are increased in size by the wandering. A third point of interest is that the Greenwich results, as I interpret them, seem distinctly to show that we do not get, by prolonging the exposure, the equivalent extension to fainter magnitudes which the well-known law would lead us to expect. The coefficient of log T in the formula comes out *not* 2·5 (the reciprocal of 0·4), but about 2·0: in other words, if we lengthen the exposure in such a ratio that we expect to get 5 magnitudes fainter, we actually only get 4. This deduction I make from the published Greenwich results after correcting them all to the same value of n, *i.e.* to comparable climatic conditions.

The Astronomer Royal. I think this is a case where it would have been well to have seen the paper before discussing it. I am glad to see that Prof. Turner has arrived at the same formula as we have at Greenwich, but I have always looked upon it as somewhat empirical and conventional, because the diameters depend a good deal upon the measurers. As to his statement that prolonging the exposure does not give the expected increase of magnitude, I disagree with Prof. Turner there, because we have found that we often get stars of magnitude 15 or 16, and this is definite evidence of the magnitudes of the stars shown on the plate of a size quite independent of the law of diameters. What Prof. Turner says about increasing the sensitiveness of the plate by exposure is very interesting, and I think that is the conclusion we came to when comparing the number of stars shown on the same plate by different lengths of exposure, because the increase in the number of stars with increased exposure seemed to be in excess of that which would be expected, and it seemed to me that the preliminary exposures increased the sensitiveness of the plate and so gave that apparent anomaly. Beyond these remarks I do not feel I can adequately discuss this subject further this evening.

Mr. Hinks. Does this formula apply to the stars at the limit of visibility on the plate, whose images do not diminish in size but are not perfectly black?

Mr. Newall. I have been interested in hearing Prof. Turner's account, and look forward to seeing it in print, because it has a close connection with the subject I brought forward at the last meeting of the Society. I confess I look on these things at the present moment altogether from the point of view of the tremor disc, because it has given so much trouble in spectroscopic work. We are nearly always dealing with under-exposed photographs in spectroscopic work, and I think we are dealing with images similar to those where the diameter is not increasing in Prof. Turner's work The whole question of photographic magnitudes seems to me to want revision, if I may make bold to say such an audacious thing. The simple numerical comparison of density of a star-image as you would expect it if the diffractional intensity held good with the density which must obtain when differences due to atmospheric tremor are taken into account is enough to show the importance of work such as Prof. Turner is undertaking.

Mr. Dyson. Does Prof. Turner adopt the visual magnitudes given in the B. D.'s as the basis of his discussion?

Prof. Turner. Mr. Hinks asks whether I have been dealing with the images of very faint stars at the extreme limit that can be seen on the plate. What I said chiefly referred to the brighter stars up to 9·5 on Argelander's scale, and their images are all pretty black. Mr. Dyson has asked what scale is adopted. For the Oxford plates it was simply Argelander's scale in the first instance (7·0 to 9·5 on Argelander's scale), to which I applied the corrections given in the Greenwich volume, which bring 9·5 to 10·1 for instance: and then having satisfied myself that this form of formula was satisfactorily correct for Oxford, other reductions were based largely on the results actually given in the Greenwich volume. As regards the point, whether one exposure on a plate affects another, I may say that when I was at Harvard last year Mr. King told me he had made experiments on the influence of subsidiary exposure, and he found that exposure to faint light certainly made the plate more sensitive, *i. e.* more receptive of faint stars: and the result was the same whether the subsidiary exposure was *before* or *after* the other. Hence if several exposures are given on the same plate, the order would not matter. I do not think the exposure of 40 minutes in the case of Greenwich plates has helped the shorter exposures for this reason:—There are three plates in the Greenwich volume which have exposures upon them of 40^m, 6^m, 3^m, and 20^s; there are three others which have the shorter exposures alone. On comparing them they give identical results. Hence I conclude that if there is anything helping the shorter exposures, it is an exposure made perhaps in making the plates and not one of the exposures made in the observatory.

The Astronomer Royal presenting a paper from the Royal Observatory, Greenwich, on "Optical Distortion of the Object-glass of the Astrographic Telescope, deduced from Measures of the Eros Photos," said:—The Society is indebted to Mr. Dyson and Mr. Davidson for this paper; but Mr. Dyson has asked me to give some account of it, and I should explain that this discussion has incidentally arisen out of the measurement of the Eros photographs. Mr. Dyson discussed these measures of the reference stars, of which a good number occur on several plates, sometimes on quite a number of plates, on successive nights; and it happens, naturally, that some fall near the centre of the field, and others further from it. In discussing and dealing with these reference-stars we have tried to keep the stars used within 50′ of the centre of the field, so as to avoid any question of distortion; but sometimes it happens there are not sufficient stars within this radius, so in these cases reference-stars had to be chosen at a greater distance, and it is these stars whose positions are discussed in the paper. The measured positions of these stars on such plates have been compared with their measured positions on plates on which they lie within 40′ of the centre, where it may be assumed there is no sensible distortion. It is an interesting question, because it shows incidentally the uncertainty of the measures of the photos of images quite irrespective of any systematical distortion. I think the best way to show that will be from tables, but as the diagram has been prepared I will give it. (Screen.) The interesting point about it is, that it shows what improvement is made in the accuracy of the measures by allowing for this distortion, and a table has been drawn up by which I think it will be shown not much is gained. (Blackboard.) It will be seen that the probable error of the measured star-places increases as we go from the centre, and this is not materially diminished by correcting for distortion even at 70′ from the centre, and it shows that stars so far from the centre should not be used if they can be avoided.

Mr. Hinks. Is the distortion in any way a function of the magnitude? Has that point been discussed? Probably the net effect of optical imperfections in the case of the bright stars would differ from that in the case of the faint stars, and I have sometimes wondered whether it is possible to get out a general law of distortion, which would really represent the effect on the measured position of the star.

Dr. Rambaut. I should like to ask what are the elongations of the images beyond the circle shown in the diagram, and how far the discordances may be due to erroneous estimate of the centres of the stars.

Prof. Turner. This is a paper which interests me very much. The figures of the Astronomer Royal seem to show that up to about 60′ the distortion is very small, and that the chief distortion comes outside a circle of that radius on the plate. That result is practically the same as the very interesting result found by Mr. Hough of the Cape Observatory. He had eight plates taken at the South Pole for discussion, and from them he seemed to deduce radial distortion. He found also other distortions, but cannot reduce them to any system, and he comes to the conclusion that the only thing he can indicate as real is this radial distortion. What struck me was that within a circle of radius 60′ the distortion is so very small that it need not be taken into account; in anything like the Astrographic Catalogue it can be entirely neglected, and I think we have every reason to be well satisfied with the results arrived at from the discussion at Greenwich and elsewhere of the extreme accuracy that may be expected in the work of the Astrographic Catalogue that has been going on for the last 12 years.

The Astronomer Royal. With regard to Prof. Turner's remarks, I agree generally with him, but I think that the adoption of so large a limit of 60′ would cause a great increase in the final probable error, as it can be seen that beyond 50′ the probable error begins to increase. Although there may not be a very great effect from distortion in the mean, there is great irregularity, and I think that is shown by the diagram on the screen. We have taken Mr. Hinks's point so far as we can, but there is nothing very definite come to. I agree with Mr. Hinks that something may depend upon the magnitude, and for that reason I think it is very difficult to assign any particular distortion. With regard to Dr. Rambaut's remark about the measurement of the elongated image, that was brought out forcibly in the case of the measures with the reflector, where we do not get an elliptic image, but a nucleus with a coma, and it is very difficult to get the true optical centre; but at Greenwich we have tried to point on the axis of the pencils. The only way to get over that is to compare the images with the different exposures. I think with our astrographic object-glass the condensation is very nearly central in the elliptic images. That was our idea when the object-glass was made, and although it is not perfect it is so near so that I think the distortion may be neglected within a moderate distance from the centre.

The President. We have a short note from *Major Molesworth* on a supposed instance of sudden change on Jupiter, which Mr. Maunder has kindly undertaken to take charge of.

Mr. Maunder. Major Molesworth, as most of you know, has his observatory at Trincomalee, in the island of Ceylon. He is a most enthusiastic observer, and finding that the atmosphere of Trincomalee was specially adapted for planetary observations, he has remained for a number of years in a place which I think a good many people would be only too glad to get out of at the earliest opportunity. This paper is upon observation of a strikingly rapid change upon Jupiter, made on 22nd December, 1903.

The President. It is a very interesting record of a rapid change on Jupiter, and, so far as I know, it is quite unique. We have in the room Mr. Phillips, who was for some time in charge of the Jupiter Section of the B. A. A., and I should like to hear whether he has had any communication upon this.

Rev. T. E. R. Phillips. I am sorry to say, owing to absence from home, I have myself been unable to make observations of Jupiter with any regularity for some time past, and I have not had any notice from any other member of the Jupiter Section of the B. A. A. with reference to this particular disturbance to which Major Molesworth refers. Possibly some reports may be sent in later.

The President. But in your experience you have never known any such sudden change?

Mr. Phillips. No. I believe there are on record a few instances in which remarkably sudden changes on Jupiter have been alleged, but possibly they have not been well established. This particular case is most interesting and remarkable, and I hope I may yet receive some further account of it.

Mr. Crommelin. As regards this question, I think in Webb's 'Celestial Objects for Common Telescopes'* there is an observation of very rapid change in a large spot on Jupiter. The observer had only left the eyepiece to get materials to make a sketch, and when he came back the huge dark spot was all gone and only a few fragments were left.

The President. The records derive great value from the fact that Major Molesworth is not only an experienced observer, but a most excellent draftsman, as I think most of you know.

The President. At the last Meeting we had a paper by Mr. Maunder in continuation of the one read previously, and now we have a third paper on magnetic disturbances at Greenwich in connection with sun-spots, which I will ask him to read.

Mr. Maunder. The paper which I am now presenting to the Society is the third of a series. The first of these dealt, as the Fellows may remember, with the magnetic disturbances which were represented in the plates of the Greenwich Annual Volumes and so covered the years from 1882 to 1903. The second paper carried back the list of disturbances to 1848. Mr. Ellis had very kindly put at my disposal a list of disturbances he had made from 1848 to 1902, and that directed me to the dates where I should have to examine the registers at Greenwich, and therefore very much shortened my work. My second paper accordingly contained a catalogue of 450 disturbances from 1848 to 1881. For these years I took out the longitude of the centre of the Sun's disc, just as in the first catalogue, and these earlier observations showed an Interval-relation—that is to say, the magnetic disturbances tended to recur at about 27 days' interval,—just as clearly as the later ones; indeed, from the earlier table I took one series of disturbances which was distinctly remarkable. In 8 successive rotations of the Sun the principal disturbances came with the return of one special longitude to the centre of the disc, and during a great part of the time there were no other disturbances at all. I think that that one series, even if it stood by itself, would go very far to establish the Interval-relation. The series began in 1865 and ran for 8 months. That was the principle sequence which I got out from those earlier 33 years; but there were two or three others that were only less remarkable. My third paper deals with a further point. When I was examining the two catalogues, after they were completed, I found that there was a distinct tendency in the time of commencement to favour certain hours of the day. Taking the two catalogues as a whole—in the first catalogue, 1882 to 1903, I found that one-fourth of the storms commenced between midnight and noon, and three-fourths between noon and midnight, and the same relation was found in almost the same proportion in the second catalogue, from 1848 to 1881. But there was a difference between the two. In the 1882–1903 catalogue there was a very sharp maximum shown about 1 o'clock in the afternoon—from 1 to 2 o'clock; and in the 1848–81 catalogue there was a still more clearly marked maximum at about 6 o'clock in the evening. It is perfectly clear that there is no likelihood of a real change like that in the character of magnetic storms from the one period to the other, and therefore I went over the two catalogues to see what the explanation of it was. The explanation of it was very obvious when one looked into it. My first catalogue was taken out not from the registers themselves, but from the reproductions that are published in the annual volumes, and those necessarily, in a great number of instances, began with noon. My eye was caught by the first conspicuous movement of the needle shortly after noon, and that consequently tended to fix the time of commencement most frequently about 1 o'clock in the afternoon. But the next question is as to what is the explanation of this unequal distribution of magnetic disturbance in the 24 hours. Examining the two catalogues a little further it was seen that these disturbances, which began with a typical sharp twitch, are very equally distributed throughout the 24 hours. So, too, are the very large disturbances; there is no striking inequality in their distribution in the 24 hours; but when we come down to storms of lower rank, active or only moderate storms, then they show a greater tendency to concentrate their commencement about 6 o'clock in the evening. Following up this enquiry, I referred to some of the annual volumes of observations at Greenwich, and especially to the Notes on the magnetic movements, and took out all the little "waves" that are described as such in the notes, and tabulating these, I found at once an extremely strong condensation of these waves at from 6 to 8 o'clock in the evening. Mr. Ellis tells me that it is a fact of which he was aware many years ago, but this idea was quite new to me, and my looking through the registers impressed the matter very forcibly upon me. There is a strong tendency from about 6 to 8 o'clock in the evening for an easterly wave to take place in declination which lasts about an hour—a very different thing from the sharp twitch which takes place at the beginning of the great storms. This is a slow simple wave towards the east which may run its course in about an hour and then cease. When a wave like that occurs on one evening, it is very frequently the case that the next evening, at the same time, there is a similar wave, but, perhaps, not so large in amount. A similar movement may sometimes follow even upon the third and fourth evenings, but to occur on two evenings is quite a common thing. These minor movements tend to show other well-marked types according to the hours of the day, besides this conspicuous one which I may call "the sunset easterly wave." Shortly after noon there is a westerly movement. It takes place in a number of small fluctuations of declination running down towards the west, and when I was making my first catalogue it was this movement which caught my eye most frequently. The question arises, do these diurnal movements in any way weaken the evidence which has been brought forward of the Interval-relation? Not in the slightest. The fact is that the evidence for the Interval-relation offered in my first two papers was an evidence still remaining after these regular diurnal movements had had their full effect. In my third paper I have also compared the storms of 9 years as registered at Greenwich with the same years as registered at Toronto. Turning over some of the reports of magnetic observatories in our library at Greenwich, I happened to come across a nice little catalogue of disturbances observed for 9 years at Toronto. I made an examination of it and it shows in those disturbances a strongly marked diurnal inequality, whilst at the same time the Interval-relation is also brought out. But the sequences from the Toronto observations are not quite the same as the sequences taken from the Greenwich observations. In that long sequence to which I have already alluded as having been registered at Greenwich there were only three or four disturbances of sufficient importance to be included in this catalogue from Toronto. No doubt if we had a complete series of the Toronto observations, we should find that each disturbance of this series was registered there, but they were of different intensities. Therefore it is quite clear that in order to bring out the Interval-relation fully and to fully exhibit its connection with solar action we should have to get, not the disturbances recorded at one observatory only, but at a number of observatories widely scattered over the face of the earth. Another point I have taken up is the distribution of disturbances through different periods of the year, At Greenwich, for instance, there is one maximum which takes place a little before the spring equinox, and another which takes place a little after the autumnal equinox. This annual inequality does not seem to be either a simple seasonal one, or one depending solely on the presentation to us of the Sun's axis. It seems to me that the complete explanation of that inequality can only be found in the same way by collating observations from many different observatories.

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* Subsequent reference showed that the observation was made by South, 1839 June 3, *vide* 'Celestial Objects,' 4th edition, page 156.

Mr. W. W. Bryant. As regards diurnal variation at different observatories, it is interesting to note that, as a rule, magnetic disturbances are rather greater at Greenwich than at Paris, possibly owing to the greater distance of Paris from the sea. This is by no means a new idea that the diurnal variation may be due to the different effect of the solar radiation on land and on sea. The comparison would be better between Greenwich and some place in Japan, for instance, where the conditions are nearly reversed as regards morning and afternoon effects of this kind.

Mr. Maunder. Mr. Ellis's catalogue was made many years ago; long before I had any idea of making this inquiry. I felt it to be a very great advantage that the collection of the storms in this catalogue had been made independently of any ideas I might have had on the subject. With regard to what Mr. Bryant has said, my position with regard to magnetic disturbances is, of course, that of an entire outsider. I approach the question from the side of a sun-spot observer, and I do not claim to advance anything about magnetic theory at all. What I was anxious to do in my third paper was to point out the reason and significance of the apparent diurnal inequality in the commencements of the storms as given in my two catalogues.

Dr. Lockyer then gave some notes on the use of Thorp gratings for eclipse work with the aid of the screen.

The President. I think that this note by Dr. Lockyer will be of considerable service to observers going out to view the eclipse, and I am sure we are very much indebted to him for bringing the subject before us. If there are any Fellows present who have had experience of the use of these gratings we shall be glad to hear it.

Mr. Newall. At the eclipse in Sumatra I used a plane grating with 14,438 lines to the inch, and, so far as I remember, it was on one of the 6-in. plates, with a ruled space 5 inches by 3 inches, and I got four photographs with it. At the end of the eclipse the photograph obtained of the "flash"-spectrum was exceedingly satisfactory. At the Indian eclipse in 1898 my visual observations with a plane objective grating convinced me that there was plenty of light to get a photograph even with the green light. In Sumatra I got a photograph of the blue ring of the corona, and I hope that in the coming eclipse with the same grating I may get good results. The grating in this experiment was used as an objective grating, much in the same way as Dr. Lockyer has described his use of the Thorp grating, only the reflected rays were used instead of the transmitted rays. It is an extremely convenient plan of getting the spectrum. My experience in Sumatra was, however, somewhat annoying. I thought it would be well to rectify one of the flash-spectra, so that the chord through the horns, either of the disappearing or of the reappearing crescents, should be at right angles to the length of the spectrum. Having made arrangements with a subsidiary mirror to get the disappearing crescents rectified, it happened that that phase was almost completely obliterated by clouds, whilst I got an excellent photograph of the reappearing crescents, which were not rectified. I hope there may be improvements in the Thorp gratings which will obviate annoyances like this.

Mr. Evershed. I should like to know if Dr. Lockyer can give us any idea of the amount of light obtained in the spectrum of the second order, and whether there is any method of avoiding fogging from the light on the other side of the grating.

Mr. Newall. I should like to ask whether Dr. Lockyer has any knowledge about reproduction of the peculiarities of reflection in the grating. It is not an uncommon thing that a [metallic] grating gives far more light in the second or in the third than in the first order of spectrum. Is he able to say whether the Thorp reproductions from such gratings reproduce these peculiarities? Any information he can give us would be of interest.

Dr. Lockyer. With regard to Mr. Evershed's question, I will explain it on the screen. As to Mr. Newall's remark, the particular grating I used was one that Mr. Thorp sent me, in which the first-order spectrum was one-fifth of the total light of grating. It was a grating which he obtained from Dr. Common. I suppose his copy really reproduced the relative condition of the original grating, but in the first-order spectrum the light from the first order was one-fifth of the total light reflected from the grating.

The following papers were announced and partly read :—

F. W. Dyson and D. J. R. Edney. "Discussion of the Observations of the Satellite of Neptune made at the Royal Observatory, Greenwich, in the Years 1902–3–4."

A. W. Roberts. "Further Note on the Density and Prolateness of Close Binary Stars."

E. Nevill. "On Hansen's Coefficients for the Inequalities in the Moon's Longitude."

Major P. B. Molesworth. "A supposed Instance of Sudden Change on Jupiter."

Royal Observatory, Greenwich. "Distortion in Photographic Images with the 13-inch Object-glass of the Astrographic Telescope." (Communicated by the *Astronomer Royal.*)

Rev. T. E. Espin. "New Double Stars."

A. R. Hinks. "On the Determination of Stellar Motions without Reference to Meridian Places."

E. W. Maunder. "Magnetic Disturbances as recorded at the

Royal Observatory, Greenwich, and their Association with Sunspots." Third paper.

A. M. W. Downing. "The Equatorial and Polar Diameters of Jupiter, measured with the Greenwich Transit-Circle, 1880."

Major P. B. Molesworth. "Report on Observations of Jupiter, 1903–4."

The following gentlemen were elected Fellows of the Society:—

Scriven Bolton, 24 Kensington Terrace, Hyde Park, Leeds.

Bahne Bonniksen, 16 Norfolk Street, Coventry.

Edward Turner Cottingham, The Limes, Thrapston.

William George Hooper, Wiverton House, Musters Road, West Bridgford, Nottingham.

Percy Marshall, Filey House, Livingstone Road, Scarborough.

Karl Pearson, M.A., LL.B., F.R.S., Professor of Applied Mathematics and Mechanics, University College, London, and 7 Well Road, Hampstead, N.W.

The following Candidates were proposed for election as Fellows of the Society:—

Walter Sidney Adams, M.A., Solar Observatory, Mount Wilson, California, U.S.A. (proposed by G. E. Hale).

Ernest Percival Cotton, Surveyor General and Commissioner of Crown Lands, Lands and Survey Department, Lagos, West Africa (proposed by W. H. Walmsley).

Rev. Alex. C. Henderson, B.D., The Manse, Delting, Brae, Shetland, N.B. (proposed by Rev. J. Spence).

Rev. Frederick John Jervis-Smith, M.A., F.R.S., M.Inst.E.E., University Lecturer in Mechanics and Millard Lecturer in Engineering, Trinity College, Oxford (proposed by H. H. Turner).

T. Hobart Pritchard, 5 Cotford Road, Thornton Heath, Surrey (proposed by T. W. Brownell).

EVENING MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

The second additional Meeting of the Session took place on Wednesday, May 3, at 8.30 P.M. The attendance was large, and the lecture was given by Prof. H. H. Turner on the subject of Total Solar Eclipses. The lecturer began by explaining eclipse-cycles, showing how any particular eclipse was repeated after 18 years, and the path of totality was shifted in latitude and westwards on the Earth's surface. Following this with the relation of circumstances of past eclipses—sometimes pathetic, as in the case of Father Perry's death in the West Indies, but often in a light and humorous vein—Prof. Turner came to the circumstances of the coming eclipse in August next, and stated what, in his opinion, were four special lines of work to be undertaken:—(1) Photographs of the corona, especially with the view of obtaining pictures of the extensions; (2) Photographs taken at points away from the central line; (3) Photographs of the corona taken through coloured screens, especially green; (4) Polariscopic examination of the corona.

The lecture comprised a brief description of instruments used in eclipse-work, and ended with a dramatic representation by several observers using the actual instruments employed on such occasions of the three-minutes' totality of a solar eclipse, the phenomenon itself being portrayed by Mr. Shackleton's mechanical lantern-slide, which has before appeared on the screen at Burlington House.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 April 26.

President: A. C. D. CROMMELIN, in the Chair.

Secretary: J. A. HARDCASTLE.

THE Minutes of the previous Meeting were read and confirmed.

The names of five Candidates were read and passed for suspension, and the election of six new Members was unanimously confirmed.

Mr. W. T. Lynn read a paper on "Dörfel and the Great Comet of 1680." The writer said that the extreme rarity of Dörfel's tractate (there was no copy of it in the library of the Royal Astronomical Society) was his reason for bringing the subject before the Association. To Dörfel belonged the great credit of having calculated a parabolic orbit for the comet of 1680 before Newton had proved that comets must move in one of the conic sections with the sun at a focus.

The President said the comet of 1680 was a very interesting one. It belonged probably to the same group of comets as the brilliant comet of 1882, a group distinguished by their small perihelion distances, their great brilliance, and their long straight tails.

A note by *Mr. J. H. Astbury* on "The New Variable 48 Aurigæ," containing letters on the subject from Mr. Stanley Williams, was next read. The writer stated that he had submitted his earlier observations to Prof. Turner, who, from inspection, suggested a period of about $3^d \cdot 6$. Later, the whole of the observations were sent to Mr. Stanley Williams, who arrived at much the same duration. Prof. Turner had communicated the discovery of the variability of the star to the Editor of the *Astronomische Nachrichten*, who had given it the provisional designation of 47.1905 Aurigæ.

The President thought it was a matter of which the Association might be proud that one of their Members had made this discovery. It seemed to be the brightest variable star that had been discovered for something like 36 years. excluding *novæ* of course. There were very few observers that could detect that any of the naked-eye stars were varying to the extent of half a magnitude.

A paper by *Miss A. M. Clerke* on "Tasks for Totality" was read by *Mr. Hardcastle.* The writer pointed out that no observation should be undertaken during eclipses that might be practicable in daylight. Prominences, since they could be seen spectroscopically and photographically recorded, should receive only incidental attention. A certain class of prominences, however, wholly evaded daylight observation. The strange spectacle of white prominences was reserved for total observations; they were effaced telescopically and spectroscopically by atmospheric glare. Little, accordingly, was known as to their nature or mode of origin. Should a conspicuously white prominence be discernible during the forthcoming eclipse, it was earnestly to be hoped that its spectrum might be photographed with the aid of a slit. Dark chromospheric markings, too, were in general only perceptible during totalities, and they were among the most enigmatical of solar phenomena. The coming eclipse would most likely be specially favourable for the detailed study of the coronal gaseous spectrum, shown by past experience to be relatively strong near epochs of spot-maxima. Symptoms of rotation in the corona would naturally be looked for in connection with the measurement of its spectrum lines. "Flash" spectrograms were sure to be abundantly secured, and they would be of crucial importance for testing the occurrence of cyclical changes in the composition of the reversing layer.

Mr. E. W. Maunder thought the Association was very much to be congratulated upon having that communication from Miss Clerke, in view of the approaching total eclipse. Of course the majority of the observers of the Association would have but small instruments at their disposal, and it became a question of very great importance how they could best utilize them. He thought it would be worth while to re-issue the suggestions that had been drawn up for similar previous occasions, a little modified so as to bring them up to date, and he hoped to be able to do so before the next Meeting. With regard to the subject of "white" prominences, to which Miss Clerke had drawn special attention, he had a very vivid recollection of the great white prominence which she had so well described in her paper—the one seen during the 1886 eclipse. It was easily seen by the naked eye, of an intense silvery whiteness, rising some 200,000 miles, a very considerable distance to the naked eye, from the Sun's limb—the most brilliant detail in the eclipse at that period. There was also the suggestion that the coming eclipse offered a chance for getting a good stereoscopic picture of the corona by having photographs taken at each end of the line of totality with identical instruments.

Mr. Newbegin remarked that Miss Clerke's paper was exceedingly interesting and would be of great assistance to all those who were going to make observations.

Mr. Maunder said that Mr. R. A. Proctor regarded the discovery of the white prominence as of great theoretical importance. He held that many of the prominences seen round the Sun were due, not to uprushes of hydrogen, but to the passage of solid or tolerably condensed particles through a hydrogen atmosphere, and he considered the white prominence was a demonstration of the hypothesis, and he considered that the vertical or nearly vertical filaments seen at times in the prominences were caused by the falling-back into the Sun of solid or nearly solid particles.

Mr. Seabroke asked whether observers might not be a little misled by generally using red light for observing prominences.

Mr. Newbegin said they wanted an alteration in their eyes. It was very much easier to see prominences with the C line than it was with D_3.

Mr. Maunder said the difficulty was not only due to their eyes, but partly also to the atmosphere. There was not the same scattering of the light in the red end that there was further towards the violet.

The President said they were certainly very much indebted to Miss Agnes Clerke for her contribution to the eclipse suggestions, and he thought that even those who had not spectroscopes could take some of her hints by looking out for the colour of the prominences.

Mr. Hardcastle showed two slides illustrating the Saros. They were intended, he said, to represent the effect of the Saros—the 18-year period—on the tracks of total eclipses.

The President said it was quite true, as Mr. Hardcastle pointed out, that they could get a very fair idea of a coming eclipse without any calculation at all by simply following preceding eclipses in the Saros cycle, but it was necessary to take account of the season of the year, because that made a difference in the rate of the north or south motion. They had also to consider whether it was an eclipse at the ascending or descending node. All those things came in and affected the rate of motion on the Earth's surface.

Mr. Seabroke gave a *résumé* of a paper by *Mr. M. E. N. Gheury* on "Rotation of the Universe as a Whole." The author stated that certain classes of stars and nebulæ appeared to frequent certain parts of the universe in layers, giving it a stratified constitution, and that as belts or stratification on the surfaces of the planets was always associated with rotation, therefore, by analogy, the universe showed evidence of rotation. Commenting upon the paper, he (Mr. Seabroke) said that the difficulty one saw was that the stratification of the universe was only the stratification we observed from our point of view, and it did not follow, because we saw an apparent stratification, that it really existed. Speaking in regard to the latter portion of the paper, in which the author spoke of certain spiral nebulæ and clusters as other universes showing evidence of rotation, Mr. Seabroke said that depended upon what were deemed the limits of our universe. Some would consider that the globular clusters and spiral nebulæ were part and parcel of our own universe.

The President said the subject of relative drifts of different stars in different regions of the heavens was very much to the fore just now. A recent paper by Mr. Dyson and Mr. Thackeray made it pretty clear that stars of different spectral types had got different proper motions. Stars of type 2, solar stars, had much larger proper motions than stars of type 1. That made it almost an established fact that our Sun was one of a cluster of solar stars and outside that there were the stars of type 1, the Sirian stars, which belonged more particularly to the Galaxy. On the other hand, the stars of type 2 were distributed much more uniformly, showing presumably that they were our Sun's nearer neighbours. In a few years, when the astrographic plates had been measured, they would have far more material for getting proper motions of faint stars than they now had, because most of them were observed for the 'Astronomische Gesellschaft Catalogue' some 30 years ago.

Mr. Hardcastle then gave a summary of a paper by *Mr. C. T. Whitmell* on "The Earth's Rotundity proved by Reflection." The writer pointed out that the image formed by reflection from a plane horizontal mirror is similar in shape to the object, but the image formed by a cylindrical mirror, of which the axis is horizontal, is contracted vertically. The paper was mainly mathematical, but a number of observations by M. Ricco at Palermo in 1886, and by M. Forel at Geneva, were quoted, in which the elliptical form of the image of the Sun was very evident, and the images of distant buildings &c. were similarly compressed vertically.

Mr. Hardcastle said he had often watched the Sun rising out of the sea in Italy, but had never been fortunate enough to see the sea so smooth as to reflect the image of the Sun.

The President remarked that it was a very curious phenomenon that Mr. Whitmell had brought before them, and he thought was not at all widely known. Of course, the refraction should be taken into account in considering these images. Refraction would modify them very much. They all knew that, owing to refraction, the vertical diameter of the rising Sun was shorter by six minutes than the horizontal. The paper itself contained a great deal in the way of mathematical reasoning which would appeal to mathematicians when it appeared in print.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The monthly Meeting was held on Wednesday, May 17, at the Society's rooms, 70 Victoria Street, Westminster, *Capt. D. Wilson-Barker, F.R.S.E.*, Vice-President, in the Chair.

Mr. R. Strachan read a paper on the "Measurement of Evaporation." He pointed out that the rainfall, evaporation, and percolation are related to each other, and that rainfall is commonly considered to form the sum of evaporation and percolation. If two of these quantities are found by experiment or observation, the other is assumed to be known. This, however, does not always hold good. A month may be very dry, and still evaporation will go on at the expense of previous percolation. A month may be excessively wet, and then there may be another item to take into account, viz., overflow.

As it is unfortunately not possible to make evaporation and percolation the subject of experiment, except at a very few observatories, the author thinks it is desirable to be able to estimate, even empirically, the probable amounts of each. By using the meteorological data published for the Royal Observatory, Greenwich, he has calculated the probable evaporation for the year 1898, which agrees very closely with the observed evaporation at Camden Square and also at Croydon.

A paper by *Dr. John Ball*, of Cairo, "On a Logarithmic Slide-rule for Reducing Readings of the Barometer to Sea-level," was read by the *Secretary*. This has been devised for the purpose of saving the time and labour usually occupied in working out the corrections from the 'International Meteorological Tables.'

Some interesting Double Stars.

[Continued from vol. xxvii. p. 237.]

Σ 2434. R.A. $18^h\ 57^m\!\cdot\!6$, N.P.D. 90° 51′.

A group of three stars, of which the two larger are Piazzi XVIII. 274 and 275 respectively.

The proper motion of Piazzi XVIII. 274 is

$$-0''\!\cdot\!060 \text{ in R.A. and } +0''\!\cdot\!20 \text{ in N.P.D.,}$$

and the proper motion of Piazzi XVIII. 275 is

$$0''\!\cdot\!000 \text{ in R.A. and } +0''\!\cdot\!08 \text{ in N.P.D.,}$$

giving a relative motion of

$$+0''\!\cdot\!060 \text{ in R.A. and } -0''\!\cdot\!12 \text{ in N.P.D.}$$

From a consideration of the micrometer measures the relative motion is

$$+0''\!\cdot\!070 \text{ in R.A. and } 0''\!\cdot\!11 \text{ in N.P.D.}$$

Hence we may conclude that the stars are most likely independent, and that the proper motions are unusually well determined.

The small star C and the star B form a binary pair. This pair was found by Struve, the pair AB being Sir W. Herschel's H. IV. 127. The measures are :—

A and B = H. IV. 127.

1899........ Angle 125°. Distance 23″·1.

B and C = Σ 2434.

1831 .... Angle 80°. Distance 1″·9.
1900 .... " 50 " 0 ·9.

The magnitudes are :—

A = 7·4. B = 7·9. C = 10·0.

Σ 1989, π^2 Ursæ Minoris. R.A. $15^{h}\ 45^{m}\ 6^{s}$, N.P.D. 9° 42′; mags. 7·1 and 8·1.

This is a most interesting star, and furnishes an excellent example of the wisdom of continuous measuring. At present it is beyond any of our instruments, Aitken noting it as round last year, after having previously made an uncertain measure. Whatever the period, the orbit is a very elongated ellipse, and much depends on the early distance measures. There is a certain amount of evidence that an elongation was reached about 1841, and since then that the distance has decreased from 0″·7 to less than 0″·1. The present interest lies in watching whether the distance increases and so extends the ellipse, or whether the companion will swing round. If the latter, the period will be about 100 years. But the real reason of introducing it here is to emphasize the necessity of measuring similar pairs. Practically from 1832 to 1873 the measures do not vary and they give the idea of an uninteresting pair; yet in a few years after its true character became evident. There are many pairs like this, and liable to neglect. The positions are :—

1834	24°	0″·6
1841	25	0 ·7
1863	20	0 ·6
1881	15	0 ·5
1895	358	0 ·2
1901	220	0 ·1

Σ 2021. R.A. $16^{h}\ 8^{m}\ 39^{s}$, N.P.D. 76° 12′. Mags. 6·7 and 6·9.

This is a star of quite a different class, being within reach of very small instruments, and must of necessity be left to them. It is one of the pairs discovered by Sir W. Herschel in 1783, and has the large proper motion of

$$+0''\cdot157 \text{ in R.A.} \quad \text{and} \quad +0''\cdot416 \text{ in N.P.D.}$$

Herschel noted the distance as less than 2″, and at present it is 4″, so that the stars are physically connected. If we ignore Herschel's measures, and merely discuss the motion since 1829, we find a rectilinear motion of 0″·023 along a direction 20°, or

$$+0''\cdot008 \text{ in R.A.} \quad \text{and} \quad -0''\cdot023 \text{ in N.P.D.}$$

But there is some evidence that the elongation has been reached, and we may expect a diminution in distance. The period would be about 350 years. T. L.

OTTO STRUVE

Am 14. *April entschlief sanft*
im fast vollendeten 86 *Lebensjahre*
Die trauernden Hinterbliebenen.

Karlsruhe, April 1905,
Moltkestrasse 5.

Such was the card announcing in brief the loss sustained by astronomical science through the death of the *doyen* of astronomers, and the last representative of those great workers who flourished in the early part of the last century.

When Otto Wilhelm von Struve was born on May 7, 1819, his distinguished father, Friedrich Georg Wilhelm Struve, was Director of the Dorpat Observatory. It was here that Otto Struve, at the age of 18, began his astronomical career as assistant to his father, in company with Fuss, Sabler, and C. A. F. Peters. He naturally had to devote his time to the completion of the work his father had planned, and which was carried out in so successful a manner—the great Catalogue of Double Stars, and its companion 'Positiones Mediæ.' This he found congenial, and it is from his work and discoveries in the branch of astronomy dealing with double stars that he will be best remembered. When Otto Struve commenced his astronomical career the question before astronomers was "The Motion of the Solar System in Space." To this he gave much attention, and some years later, in 1841, from a discussion of some 400 stars, he arrived at a conclusion practically in accord with Sir W. Herschel, and opposed to the generally accepted view. His results are in agreement with the now accepted values. In 1842 he was engaged in observing the total solar eclipse, and showed his keenness as an observer by noting the great brilliancy and extent of the corona, and he was also amongst those few who noted the solar prominences. But his chief work was amongst double stars. He had the use of the great Fraunhofer telescope of 15 inches aperture, and with it he not only measured a large number of Struve pairs, but discovered others of a more difficult type. In 1843 his discoveries were published in a catalogue under the title of 'Catalogue de 514 Étoiles Doubles et Multiples découvertes sur l'hémisphère céleste boréal par la grande lunette de l'Observatoire Central de Poulkova.' In 1850, when this catalogue was revised and corrected, an additional 16 doubles were added. Others were afterwards found, making in all 547. Many of these are most interesting—we need only mention γ^2 Andromedæ and δ Equulei. For his work in this direction he was elected an Associate of the Royal Astronomical Society in 1848, and received its Gold Medal in 1850. Another subject which he investigated, but was not so fortunate with, was the changes in the Rings of Saturn. He came to the conclusion that the inner ring was approaching the planet at the rate of about 60 miles per annum, and that in 300 years the bodies forming the ring would be precipitated on to the planet. Recent measures do not uphold this theory, but the nomenclature he adopted is that still in use.

Wilhelm Struve retired from the Directorship of the Poulkova Observatory in 1861, and Otto Struve succeeded him. For some time he gave much attention to nebulæ, but perhaps the best known fact in connection with this period is his announcement in 1873 of the discovery of a satellite to Procyon. This, however, could not be seen by Newcomb with the 26-inch Washington refractor, and Struve accepted it as an optical illusion. How it was finally found by Schaeberle with the 36-inch Lick refractor is matter of history. In 1874 he announced his discovery of the disturbed motion in the system ζ Cancri, and showed that it could be due to the presence of a dark body. Subsequent measures confirm his views in a most remarkable degree. In 1879 he visited the United States, and contracted with Alvan Clark for a 30-inch refractor, which was finally installed at Poulkova in 1884. Three years later, in 1887, he had the felicity of celebrating the 50th year of his service to astronomy and the 25th year of his Directorship of Poulkova. He retired in 1890, and spent most of his time at Karlsruhe, his ancestral home.

Otto Struve was twice married, but survived his second wife some years, and leaves two sons—Hermann, the elder, being now the Director of the Berlin Observatory and, like his father and grandfather, a Gold Medallist of the Royal Astronomical Society. The younger, Ludwig, is Director of the Charkow Observatory.

T. L.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Hevelius and his Comets.

Gentlemen,—

Hevelius was, I believe, the first astronomer whose cometary discoveries amounted to four in number. The orbits of all these bodies were computed by Halley, and in reference to one (which was discovered on the 3rd of February, 1661), finding that its elements bore a resemblance to those of one discovered by Apian (Bienewitz) in 1532, he suggested that they might be identical. But, in consequence of the uncertainty of the elements of that of 1532, he expressed this suggestion (as I remarked in my letter "Prediction and Non-prediction," *Observatory* for December 1891, vol. xiv. p. 420) very doubtfully and unlike his confident prediction of the return of the comet of 1682 which had been observed by Kepler in 1607. It seems most probable that the orbit of the comet of 1661 was parabolic; and the same may be said of the other three comets discovered by Hevelius, the dates of which were 1652 December 20, 1672 March 2, and 1677 April 27 respectively, being in each case the only comet seen during its year.

When the comet of 1652 was discovered (December 20, New Style), it appears that it had passed its perihelion more than a month. It was situated in the constellation Orion, a little higher than Rigel (or Regel, as he spells it). Hevelius describes the head as being round, but of small luminosity, "Caput erat raræ magnitudinis, vix aliquantò Lunâ plenâ minus," adding that the tail or beard extended six or seven degrees in length. He continued his observations until the 7th of January, 1653, by which time it had become very difficult of discernment, though he succeeded in obtaining a glimpse of it with a telescope on the 10th of January. When last observed it was in the constellation Perseus.

The comet of 1672 was discovered by Hevelius on the 2nd of March (his dates are in New Style), when it was not far from the right foot of Pegasus, from which position he followed it through Andromeda and Taurus to the head of Orion, where it was situated on the 20th of April. According to Halley's calculation, it had passed perihelion on the 1st of March, the day before its discovery. Hevelius says that its head was not large as seen with the naked eye, but "satis amplum" as seen with a very long telescope, equalling the disc of Jupiter, "colore tamen subflavo, vel potius sublivido"; the tail was at no time so great as two degrees in length. Dörfel published a small "Bericht" at Plauen on this comet, but it does not appear from this that he observed it himself; indeed the treatise * contains little but jeremiads of warning as to the presumed significance of this celestial visitant, particularly as Eichstadt had, on the appearance of the comet of 1652, put forth a conjecture that something very remarkable would take place twenty years afterwards, which Dörfel appears to think was confirmed by this new cometary appearance. At the end of his "Bericht" he narrates a remarkable appearance (evidently meteorological) which he had noticed on the Sun whilst setting on the 28th of March. In 1680, however, he did observe the great comet of that year (commonly called Newton's Comet), and his fame depends upon the fact that he considered that his observations (of which he gave an account in a special treatise) completely confirmed the view expressed by Hevelius as a conjecture, that comets move in parabolas with the Sun in the focus. Great credit is undoubtedly due to him for this, as his treatise was published some years before the appearance of the 'Principia.' As it is very rare, so that Delambre did not succeed in seeing a copy of it, I ventured to present a short abstract of its contents to the April Meeting of the British Astronomical Association.

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* It is not mentioned either in Lalande's 'Bibliographie' or Poggendorff's 'Handwörterbuch,' and, indeed, it would be difficult to call it an astronomical treatise.

The last comet discovered by Hevelius was that of 1677, which was seen for only a few days before and after its perihelion passage, afterwards calculated to have taken place on the 6th of May. Hevelius discovered it (as before mentioned) on the 27th of April, and his last observation of it was made on the 8th of May. Those of Flamsteed at Greenwich, of Picard and Cassini at Paris, and some others, were even fewer and less extended.

It was in the year 1679 that the observatory of Hevelius was destroyed by fire in his absence. Nevertheless, by the end of the following year he had furnished himself with means for resuming some of his astronomical work; he observed the great comet of 1680 on the mornings of the 2nd, 3rd, and 4th of December, and saw it again on the evening of the 24th of that month (after its perihelion passage on the 17th), following it until the 17th of February, 1681. His observations of this comet were published in the 'Annus Climactericus,' which appeared in 1685; he died on the 28th of January, 1687. The true Polish form of his name is Hewelke (*Anglicè*, a hillock or little hill). The 'Prodromus Astronomiæ' did not appear until 1690, three years after the author's death.

Yours faithfully,

Blackheath, 1905, May 2. W. T. LYNN.

The Visibility of the Helium D_3 *Line in the Solar Spectrum.*

GENTLEMEN,—

From recent reports in your valuable Journal, I gather that the visibility of the helium D_3 line has been under discussion at the March Meeting of the R. A. S. As it happened, the same subject was under notice at the March Meeting of our Manchester Astronomical Society a little over a week before, also with special reference to the spectroscopic phenomena seen during the passage of the large spot-group from January 28 to February 11, and particularly those of February 2 and 11. From personal observations made on all days of visibility, I find Prof. Fowler's account exactly corresponds to mine, and have only to add that, when the disturbance disappeared in the west on February 11, the D_3 line was a conspicuous feature as a dark chromospheric line, just like any other of the dark absorption lines, immediately abutting on the bright chromospheric reversal. It extended all along its bright *confrère*, just within the latter, as a strong dark line, and reached from the southern extremity of the disturbed area to the northern one. As I observe chromospheric phenomena very often with a *curved* slit, in diameter equal to the Sun's focal image, such an appearance of the D_3 line, if at all present, must show to best advantage. It might easily escape detection in a radially-set straight slit, and even with a tangentially set one, although the latter position is somewhat better than the radial one.

Previous and recent experience induce me to assert that the appearance of D_3 as a dark line, or even as a bright reversal on the disc of the Sun, is of more frequent occurrence than is generally believed to be the case, and as one might judge from the remarks of Messrs. Newall, Evershed, and others. It is seldom absent in a really agitated solar disturbance, and just yesterday again, about 3 P.M., it was strikingly in evidence in connection with the considerable disturbance near the south limb which happened a few days ago. Immediately in the rear of the leader-spot, the D_3 line made its appearance near a very bright hydrogen reversal, which, when the slit was opened, divulged an enormous prominence rushing upwards towards the observer, causing the hydrogen lines to be reversed and distorted in all sorts of complicated ways. Where Hα was most brilliant, D_3 yielded a bright image of the prominence, and close to it, extending in a direction tending towards the rear spots, D_3 could be seen for a considerable distance as a dark line of knotty appearance. The whole display was on such a strong scale that anybody even less conversant with the instrument than I could not have missed it. After half an hour the display waned, but the spot in question, viewed in the ordinary way, showed unmistakable traces of violent agitation prevailing at its rear, and indeed to-day's appearance of it shows some most striking changes there still in progress.

As on previous occasions, when suspecting the presence of D_3 as a dark or bright line, I used the third order of the diffraction spectrum yielded by an excellent original Rowland grating and a rather high-power eyepiece. Moreover, the slit is considerably opened, so as to enable me to see any possible prominences projected on the Sun's disc. In yesterday's case, once sure of my observation as to the strong presence of D_3, I narrowed the slit again, and then noticed the line distinctly partaking of some of the simultaneous distortion of the hydrogen lines, a phenomenon I never had the pleasure of seeing before. Amongst the thousands of prominences and line-distortions I have witnessed, I have up to yesterday never seen a *bona-fide* case of the distortion of the helium line, whether at the edge of the Sun or within its disc. I am afraid that the inherent difficulty of these particular observations deter many from attempting them and from watching carefully, but I never miss looking for the line when there seems a chance of success present. No doubt my persistence in these particular observations enables me to say with perfect confidence that the phenomenon is far more frequently to be seen than is generally believed, and I always make special tests to prevent any deception and should never hesitate to label a case as "doubtful" when I am in the least uncertain. Never before did I regret not having the access to a photographic camera so much as yesterday, and I only trust that other observers also saw what I did, as the next best evidence to that of the photographic plate is the support and confirmation by brother-observers.

Yours very respectfully,
ALBERT ALFRED BUSS.

2 Lansdowne Terrace,
Grosvenor Square,
Ashton-on-Mersey,
1905, May 14.

Halley's Comet and its Discoverer.

GENTLEMEN,—

The note which was kindly inserted in the *Observatory*, March (xxviii. p. 141), regarding a bibliography of Halley's Comet has elicited some very interesting responses. Among them is a letter from Herr Betthold Cohn, dated at the Kais. Univ. Sternwarte, Strassburg, 1905 March 14, from which an extract is given below:—

"There are two places in Jewish ancient literature where the vision of a comet is mentioned. The first is an account from the historian Josephus (*bella jud.* 66–85), and refers to the time between 10 to 20 P.C. The other notice is found in Talmud Horijoth 10 *a*; there a wise man says that a star comes *every seventieth year*, and that he expects to see it about 90 P.C. It is true that these dates are not the same as in Galle's Tables of Comets, but it is impossible to say if the Chinese sources by Galle are reliable."

The last italics are mine. It is remarkable to find documentary evidence of the existence at so early a date of the idea that a comet possesses orbital motion, or at least reappears at stated intervals. Is it not a startling coincidence that the period of seventy years is specified? Is Halley thus to lose some of his laurels as being the first to predict or even to attempt to predict the return of a comet? I should like to see, with the Editors' permission, a discussion of this point. The priority of Halley's arithmetical demonstration of the problem will never be denied. Dr. Hind traced the comet of Halley to the year 11 B.C., according to the Chinese annals.

Your American readers will be pleased to learn that a copy of

Halley's 'Astronomical Tables' (4to, London, 1752) is in the Hon. John S. Lawrence's private library, Grand Rapids, Michigan. An explanation of the probable editorship of this rare work (of which there is no copy in Chicago) may be found in the London *Notes and Queries*, ninth series, xi. p. 464 (June 13, 1903). The book is especially interesting at the present time because it contains a (revised) English version of Halley's 'Synopsis of Comets' (*cf.* Phil. Trans., abridged by Hutton, v. p. 201, note: London, 1809).

Halley's extensive acquaintance with European scholars, and his quality of making friends, receive further proof from two items in 'Documents concerning Swedenborg,' edited by R. L. Tafel (London, 1875). Swedenborg refers to Halley in a letter dated London, August 15, 1712, to Ericus Benzelius, in connection with Swedenborg's discovery "for finding the terrestrial longitude by means of the Moon," Halley admitting to him, orally, that his (Swedenborg's) method was the only practicable one ('Documents concerning Swedenborg,' Decument 44, p. 222). In another place Swedenborg says he was told by Halley that he (Halley) was the first who examined the variation of the pendulum under the equator. Swedenborg speaks of the jealousy between the French and English mathematicians and of the contention that Cassini's paper was written before Halley's expedition to St. Helena, &c. (*ibid.* Document 45, p. 227). Concerning the experiment of the pendulum, Sir David Brewster called attention to the fact that "this was first observed by Richer, who found that a clock regulated to mean time at Paris lost 2′ 28″ daily at Cayenne" (*cf.* 'Life of Newton,' vol. i. p. 324, note, edition of 1855).

1 Park Row, Chicago, U.S.A.,
1905, March 28.

Yours faithfully,
EUGENE FAIRFIELD McPIKE.

P.S.—Can any of your Parisian readers state whether or not there still exists (presumably among the collections of the Academy of Sciences) the original MS. memoirs of Halley, by Folkes, prepared for the use of M. Mairan in his 'Éloge de M. Halley'? Similar memoirs of Newton were found and published; therefore those of Halley, being of a later date, may yet be recovered. Knowledge of their existence would be of value.

OBSERVATORIES.

OXFORD UNIVERSITY.—The main fact given in Prof. Turner's Report for the year ending 1905, May 30, is that the funds for printing the Oxford section of the Astrographic Catalogue have been supplied. It was stated in last year's Report that the University were unable to find all the money, but, thanks to the intervention of the Royal Society, the Treasury have agreed to contribute half the cost provided the total does not exceed £2000.

The University has decreed a like contribution. The staff engaged on the astrographic measurement has been disbanded, and during the past year the diminished staff has been occupied on several miscellaneous matters: revising and preparing for press the Astrographic Catalogue; measuring some Eros plates, the results of which have been sent to Mr. Hinks for incorporation in his work; in the editing of the Rousdon variable-star observations; and preparing for the solar eclipse which the Director and Mr. Bellamy go to Egypt to observe.

An International Conference on Solar Research, similar to that held at St. Louis last year, will be held at Oxford during the week Sept. 25–30. New College has offered facilities for this.

PUBLICATIONS.

OUR STELLAR UNIVERSE *.—It is proverbially philosophical to look at everything from different points of view, and that is just what the author of this book has done with the Universe. He has supposed himself transported through a distance corresponding to five hundred light-years from the Sun in the direction of 18 hours Right Ascension, and looks on to the universe of stars with a pair of eyes one hundred and seven light-years apart. On this assumption, from knowledge of the parallaxes and magnitudes, he has made diagrams—one for each eye—of the stars as they would be seen from that distance, which can be put in a stereoscope and so made to appear in three dimensions. It is curious to see these star-maps thus drawn from a distant point of view. They are unlike any known constellations. Our Sun is a speck in the middle of the picture, with Sirius quite close and α Centauri and Procyon not far away, which seems remarkable, but consideration will show that it is reasonable.

Mr. Heath has also another way of representing the Universe; he has taken a section through the Sun, perpendicular to the celestial equator, in the direction of 0^h and 12^h Right Ascension, and supposed the brighter stars projected on this plane with no allowance for foreshortening. Further, as a sort of *memoria technica*, the background of the plan is a sketch-map of the British Isles, and the scale of stellar distances is taken so that one light-year is represented by one mile on the map; and since the Sun is placed at Greenwich as the origin, α Centauri, being 4·34 light-years distant, will be found at Bromley, Sirius about Dartford; it happens, curiously enough, that five large stars of the Great Bear find themselves on the Dogger Bank. This is altogether a very ingenious book and not uninstructive.

THE 'CAMBRIAN NATURAL OBSERVER.'—Mr. Arthur Mee has kindly sent us a copy of this little annual for 1904, which he edits so carefully, which is no less interesting than those of former years. The C. N. O., besides being the record of the Astronomical Society of Wales (now in the tenth year of its existence), is intended as a chronicle of natural phenomena occurring in or observed from the Principality in general and South Wales in particular, so that besides a general record of the weather of the year taken month by month at the Haverfordwest Station, it contains short accounts of afterglows, whirlwinds, mirages, and such out-of-the-way things. There is given one remarkable instance of the last-mentioned phenomenon. On May 29 residents at Langland Bay looking across the Bristol Channel saw a dense bank of white mist, and on it was portrayed a perfect representation of the whole run of the coast-line on which they stood for thirteen or fourteen miles—from Worm's Head nearly to Mumbles.

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* 'A Road-Book to the Stars.' By Thomas Edward Heath. King, Sell, & Odling, Ltd.: London, 1905. Price 5s.

NOTES.

Comet Notes.—The orbit of Comet *a* 1905 (Giacobini) appears to differ sensibly from a parabola. Messrs. Crawford and Maddrill have deduced the following elliptical elements from Lick observations of March 27, April 7 and 23 (Lick Bulletin, No. 76):—

T	1905 April 4·0439 G.M.T.	
ω	358° 12′ 17″	1905·0.
☊	157 25 27	
i	40 13 2	
log a	1·5767	
log e	9·98696	
q	1·1147	
Period	231 years.	

Approximate ephemeris for Greenwich Midnight.

	R.A.	N. Dec.	Brightness.
	h m s	° ′	
June 3	11 41 58	48 11	0·20
7	11 59 7	47 15	0·18
11	12 15 9	46 11	0·16
15	12 30 15	45 2	0·14
19	12 44 40	43 49	0·13
23	12 58 43	42 34	0·11

The brightness on March 27 is taken as unity. On May 1 E. Bianchi described it as extremely faint, with a nucleus of magnitude 13·5.

Comet 1904 I. was observed by J. Palisa on April 28 last, but was described as scarcely visible. Its distance from the Earth was then 5·06.

Ast. Journ. No. 573 contains definitive elements of Comet

1903 III. (Grigg) by Henry A. Peck. The comet was only under accurate observation from 1903 April 27 to 1903 May 28, and the resulting orbit is subject to considerable uncertainty :—

T	1903 March 25·4203	±0·0028	
ω	184° 57′ 1″	±1′ 36″	1903·0.
☊	213 8 3	± 8	
i	66 29 36	± 2	
log *q* ..	9·69847	±0·00026	

A. C. D. C.

Minor Planet Notes.—The following planets have been discovered at Heidelberg :—

Planet.	Date.	Discoverer.	Magnitude.
QH	1905 April 3	Wolf.	13·3
QK	6	Götz.	12·0
QJ	May 7	Wolf.	11·7
QL	7	„	13·0
QM	9	Götz.	12·5
QN	9	Wolf.	12·8

QJ may be identical with 480.

Planet PS has been named Peraga. A. C. D. C.

The Total Solar Eclipse of August 30.—In addition to the arrangement of the official expeditions published in our number of March last we hear that some other observers will occupy stations as follows :—

The Canadian Government have organized an expedition under Dr. King, the Director of the newly-established Observatory at Ottawa, to be located about 100 miles north of the Strait of Belle Isle, on the coast of Labrador. Mr. and Mrs. E. W. Maunder have been invited by the Canadian Government to take part in the expedition as their guests. The invitation has been accepted. Mr. Maunder will take the Dallmeyer coronagraph of 4 inches aperture, which gives an image of the Sun 2·4 inches diameter, and the 4-inch Abney lens, which gives an image of the Sun 0·3 inch diameter, both of which have seen service in previous eclipses. Mrs. Maunder will use a stigmatic lens of 1½ inches aperture and a Goerz lens, exactly similar to one which Prof. Turner will have in Egypt, both being lent by Messrs. Goerz—the object being to obtain duplicate pictures of the corona from the extreme ends of the line which may combine stereoscopically.

The track of totality passes over no less than fifteen Jesuit colleges or houses of the Society in Spain, and this religious body, which seems to us so much associated with eclipse work and solar physics generally, will participate largely in the observation of the eclipse. Father Cortie will possibly go to Tortosa, near the mouth of the Ebro, where Father Cirera has established a first-class astrophysical observatory: totality here lasts 2·6 minutes. If Father Cortie does not go here he may go to Palma, in Majorca, where the Spanish Jesuits have a residence: totality 3 minutes. A party from the Jesuit College of Grenada will place themselves at a college at Carrion de los Condé. Father Cortie will use a 4-inch lens of 20 feet focus, lent by the Royal Irish Academy, fed by a coelostat, to take photographs of the corona, and a Newtonian telescope with a 7-inch mirror, and with a small grating replacing the small reflector, for observing the flash-spectrum.

The Lick Observatory, through the liberality of Mr. W. H. Crocker, is organizing three parties, one of which will be on the coast of Labrador, a second in Spain, and a third in Egypt.

The U.S. Naval Observatory is also preparing to send out three expeditions—the first to one of the islands of the Columbretes group, off the coast of Spain, another to a station near the edge of the shadow-path, probably near Valencia, and a third on the central line in Tunis.

The German Government will probably also send an expedition to an island of the Columbretes group.

Mr. Franklin-Adams will probably set up his telescope on the Atlas Mountains at a considerable altitude above sea-level. Mr. Thwaites, Rev. T. E. R. Phillips, and several members of the British Astronomical Association are going to Burgos.

Prof. Porro, of the Turin Observatory, is going to Palma in Majorca, and no doubt there will be many other visitors to this island, which is a pleasant place for a summer holiday.

Palma is fairly accessible. An Italian Steamship Company—MM. P. Gagna, of Genoa—has arranged an excursion, leaving Genoa in the evening of August 23, to arrive at Barcelona on August 25 and to stay there a day or two for the purpose of seeing the city, and reaching Palma on August 28. Passengers live on the vessel all the time, and leave the island on August 31, arriving at Genoa on September 2. The charges, including food and wine for the trip, range from 200 to 300 lire (£8 to £12), according to cabin. There is another route *via* Marseilles, by which one would arrive at Palma on August 24 and leave on September 2, and the cost, including living at a hotel, would probably be a pound or two more than that above quoted.

M. Trepied, Director of the Algiers Observatory, is already establishing an eclipse observatory at Guelma, in Algeria, where the weather prospects are thought to be very favourable.

MAGNITUDE EQUATION IN RIGHT ASCENSION. — A good opportunity has presented itself for the determination of this quantity, which, it is almost needless to explain, is the technical name for the variation of the personal equation with the magnitude of the star observed, to which it is supposed some, or perhaps all, observers are subject. The places of certain stars to be used as reference-stars for the reduction of the Eros photographs have been observed at thirteen observatories. The observations at each were published in Nos. 8 and 9 of the series of Eros Circulars, and in No. 11 these were combined to form a catalogue of final places. Now Mr. Tucker, of Lick, by forming the differences of the original places found at each observatory from the final places and tabulating them according to magnitude of the stars, has deduced a magnitude equation for each observatory, and as two lists of stars were used, he has treated each separately. The tables of results, which are too long to give here—they may be found in Lick Observatory Bulletin No. 72—are interesting. In the series of differences for different magnitudes for some of the observatories the regular progression is quite apparent, but there is no great similarity between the size or sign of the equation for different observatories. At San Fernando, for example, the rate of change for one magnitude is $-0^{s}\cdot017$, at Königsberg it is $+0^{s}\cdot015$—the positive sign denoting that the faint stars are observed relatively later than bright stars. There is also no particular agreement between the equation as determined at one observatory from the two lists of stars. At Königsberg the correction for unit-magnitude deduced from the first list is $+0^{s}\cdot015$, from the second it is $+0^{s}\cdot003$; the similar quantities at Paris are $-0^{s}\cdot013$ and $-0^{s}\cdot006$. At Lick an independent determination of magnitude equation has been made by the use of screens, and at Königsberg observations of transits have been made by the use of a clock-driven micrometer, which are assumed to be free from magnitude equation, and so, by comparison with other observations, furnish a value of the magnitude equation. In both these cases Mr. Tucker's results are confirmed. Finally, he finds the discordances between the first and second lists to be due to the want of a correction for magnitude equation, which should be applied to the normal system of right ascensions of the second list. When this is applied, certain systematic differences between M. Lœwy's final places and the places of the same stars found by Mr. Tucker, which have been suspected (see *Nature*, 1904 December 15), are seen not to exist.

A NEW NAKED-EYE VARIABLE STAR.—Mr. T. H. Astbury, of Wallingford, a member of the Variable Star Section of the B. A. A., has had a rather successful find. At the request of the Director of the Section he has kept a watch on a certain region of the Milky Way with the idea of detecting any possible "Nova." During the course of the watch—by naked eye—he suspected that the star 48 Aurigæ was variable, and then naturally proceeded to make more accurate observations. He communicated with Mr. Stanley Williams, who has also made a series of observations, and both agree that its brightness varies from 5·0 to 5·6 in a period of about 3·8 days. The light-curve is of the β Lyræ type. This is the brightest variable that has been discovered for about thirty years.

PHOTOGRAPHING THE CORONA WITHOUT AN ECLIPSE.—M. Hansky states in a recent number of the *Comptes Rendus*, No. 12, that he has photographed the corona of the uneclipsed Sun from the observatory on the top of Mont Blanc. He seems to have taken the photographs through a screen which absorbed all rays more refrangible than 660 $\mu\mu$, so that the plates were affected only by the red rays, which are very intense in the solar corona. The direct rays from the photosphere and chromosphere were shut off from the plate by interposing a black disc. The twelve negatives, taken at short intervals, showed halos of the same form, which is taken to imply that they were caused by the Sun; but they were evidently not very visible, for the specimens exhibited to the Academy were obtained from these by repeated copying and intensification.

THE VARIATION OF LATITUDE.—It need scarcely be again mentioned that systematic observations are being made at six specially established observatories round the world to determine the variation of latitude, and that Prof. Albrecht publishes the provisional results year by year. Those for 1904 have lately appeared in *Ast. Nach.* 4017. It is difficult to describe the wandering of the North Pole without a diagram, but some idea of it may be gained from the statement that in 1900, when the observations began, the pole was about 5 feet from its mean position, that since then it has described a rough spiral, and in 1903 its path might be described as a circle of about 20 feet radius. There are signs from last year's observations that the pole is retracing its path inwards towards its mean position.

THE ROYAL SOCIETY CONVERSAZIONE.—There was not very much astronomy this year. Messrs. Nichols and Hull exhibited a vacuum-tube to illustrate the repulsion of comets' tails by the Sun. Prof. Schuster showed an échelon spectroscope, constructed by Messrs. Hilger, which consists of 33 plates, and has a resolving power equal to that of an ordinary grating of 329,000 lines in the first order. Mr. T. E. Heath exhibited his stereoscopic pictures of the universe mentioned on p. 258 of this number. An old clock by Mudge was on view, and Messrs. Casella showed a device by which the circle of a theodolite was read by a micrometer tangent screw.

JUPITER'S SATELLITES.—Two recent Harvard Circulars, Nos. 95 and 97, relate to these bodies. The first describes some photometric observations made by Prof. Wendell to determine the possible variability of the four older satellites. The brightnesses of the four satellites were compared one against another, but the objects always ranged as to brightness in the order iii. i. ii. iv. Circular No. 97 gives a list of the photographs of the major planets that exist at Harvard, and states that among them are a series of Jupiter, dating from 1893, which will be carefully examined to detect the sixth and seventh satellites when orbits of these are found from observations made elsewhere. It is expected that, as these objects are of magnitudes 14 and 16, there will be no difficulty in finding them when their approximate places are known.

THE TIME OF INDIA.—It is stated in the Report of Mr. Moos, Director of the Colaba Observatory, Bombay, for 1904, that the time-ball is dropped every week-day at $7^h\ 51^m\ 15^s{\cdot}7$ local time (3 A.M. Greenwich time), which gives reason for thinking that India will join the Universal Time system of hourly meridians instead of using the time of the meridians 5½ and 6½ hours fast on Greenwich as has been suggested.

THE JACKSON-GWILT GIFT.—Last February, the Royal Astronomical Society bestowed the Jackson-Gwilt Medal and gift of twenty-five pounds on Mr. John Tebbutt, of New South Wales. From the *Sydney Morning Herald* of April 19 we learn that Mr. Tebbutt has presented the money to the local branch of the British Astronomical Association, of which he was first President. Mr. Tebbutt remarked that he should preserve the Medal as a memorial of the Council's appreciation, but he thought it appropriate that the money should be devoted to the promotion of astronomy.

PROF. GEORGE HALE has resigned the Directorship of the Yerkes Observatory in order to give his whole time to the new Solar Observatory on Mount Wilson, California. Prof. E. B. Frost has been appointed Director of the Yerkes Observatory. Prof. Wadsworth has resigned the Directorship of the Allegheny Observatory in order to take up a non-astronomical post, and has been succeeded by Mr. Frank Schlesinger.

AN earthquake shock was felt in the Vale of Llangollen, North Wales, about 1.40 A.M. on May 1. The disturbance lasted about four seconds. The river Dee, which runs through the neighbourhood, rose several feet during the night.

THE next Meeting of the Royal Astronomical Society will be on Friday, June 9; of the British Astronomical Association on Wednesday, June 28.

FROM AN OXFORD NOTE-BOOK.

A MONTH or two ago, in referring to the appearance of a translation of Poincaré's brilliant address at St. Louis, I did an unintentional injustice to the translator, Professor G. B. Halsted. His work in this capacity is certainly open to criticism; but I did not realize at the time what vigorous English he could write on his own account—good enough to relegate these other little eccentricities to the background or beyond it. His own address at St. Louis, "The Message of Non-Euclidean Geometry," is a fine free piece of exhortation which is delightful to read; opening with the words

> The great Sylvester once told me that he and Kronecker, in attempting a definition of mathematics, got so far as to agree that it is poetry;

and full of picturesque metaphors worthy to find a place under this heading. Take, for instance, the comparison of Euclid with the mighty Niagara, which

> has been just so, precisely the same, for centuries, for ages, for thousands of years. . . . . In the flood of new discovery and rich advance recorded in books whose mere names would fill volumes, we ask ourselves how any one thing can be permanent? Yet looking back, we see this Euclid not only cutting his resistless way through the rock of the two thousand years that make the history of the intellectual world, but, what is still more astounding, we find that the profoundest advance of the last two centuries has only served to emphasize the consciousness of Euclid's perfection.

INCLUDED in the address is the following gem of quotation from "Wenley's criticism in *Science* of McCosh's disciple Ormond":—

> We may well doubt whether a thinker, standing with one foot firmly planted on the Rock of Ages, and the other pointing heavenward, has struck the attitude most conducive to progress.

What a fine subject for a bronze statuette!

THE mention of non-Euclidean geometry reminds me of the following question and answer:—

> Q. State Euclid's axiom about parallel lines. Is it a real axiom?
>
> A. That two parallel straight lines cannot meet. It is proved afterwards by the help of it.

POINCARÉ's address incidentally introduced me to his book 'La Science et l'Hypothèse' (E. Flammarion, Paris: there is now an English translation for those who prefer it), which I venture to describe as the most exhilarating book one is likely to meet in a sensible fraction of a lifetime. Those who know it already need no recommendation; for those who do not, perhaps no better brief illustration of its merits can be given than the following sentence from the Introduction:—

> Pour un observateur superficiel . . . . les vérités mathématiques dérivent d'un petit nombre de propositions évidentes par une chaîne de raisonnements impeccables; elles s'imposent non seulement à nous mais à la Nature elle-même. Elles enchaînent pour ainsi dire le Créateur et lui permettent seulement de choisir entre quelques solutions relativement peu nombreuses. Il suffira alors de quelques expériences pour nous faire savoir quel choix il a fait.

THE tenth satellite of Saturn has not fitted into the place left for it in the series quoted two months ago, but there is still hope. There are also other possible series for the distances of satellites;

e.g. in ***Astr. Nach.*** No. 3614, p. 219, the following empirical laws of distances were suggested for the systems as known at that time:—

For Jupiter ...... distances $= 2{\cdot}7 + 3n$.
For Saturn ...... distances $= 1{\cdot}6 + (1{\cdot}5)^n$.

The curious may try whether the new ones will fit in.

New celestial objects of interest are multiplying rapidly; I do not now speak of two new satellites to Jupiter and two to Saturn, but of 843 new variables in the lesser Magellanic Cloud! Miss Leavitt has discovered many times more variables than all the rest of the world put together. Those of us who have been idle will now be able to find something to do in watching these new variables; but we begin to wonder where she will stop. It seems but yesterday since she was content and even gratified with tens or hundreds; now she approaches the thousands: will she count by millions soon?

It is not, I trust, an impertinence to comment upon the valuable astronomical work done by ladies recently, especially by the corps of workers at Harvard, with Mrs. Fleming at their head. [May we not hope that an account will one day be published of the origin and development of this very successful enterprise?] But this is by no means the only instance of the value of woman's work. Well may a somewhat scornful finger be pointed (as it was recently) at the following sentence from Airy's Ipswich Lectures (1848):—

> The Lectures will be, therefore, of what I may call a mathematical kind. But in speaking of this, I beg that the ladies present will not be startled. I do not mean to use algebra or any other science such as must be commonly of an unintelligible character to a mixed meeting.

Turning over some of Professor Barnard's recent letters, I came across the following paragraph in one dated 1904, Dec. 14. I have seen no published reference to the incident as yet; and as Professor Barnard's expedition to Mount Wilson has probably delayed his completion of the calculations, I hope he will forgive this brief reference to a double observation of very considerable interest:—

> On the night of November 15, Mr. Sullivan and I had a great stroke of luck. Professor Frost had a spectrogram to make near the Orion Nebula, and Mr. Sullivan had the small camera on the end of the 40-inch in operation. I had the "Bruce" telescope on the nebula. We did not know until the next day that we had, by the sheerest accident, been pointing at the same place, *and we both got the same meteor*, a Leonid. Though there is only 400 feet between the two telescopes, there was a clearly marked parallax. I have very roughly got out the parallax as 0′·8, indicating a distance of some 200 miles, or 127 miles above the Earth. This will be lessened, however, by taking into account the 75 feet elevation of Mr. Sullivan's camera above the "Bruce." But I am going, of course, to have the parallax carefully worked out.

Acrostics are a luxury of the rich in leisure, but a couplet in an acrostic which caught my eye the other day seemed specially to challenge the resourcefulness of an astronomer. The initial and final letters were obviously N and C and the "light" was as follows:—

Far-fetched—at least, seems fetched from far:
Just fancy! Jelly from a star!

After puzzling for a week I was fain to look through the dictionary, though it is not every dictionary that will do.

For the sake of anyone who cares to try to find the solution, I will put the answer at the end of these notes.

And I will similarly defer the answer to the following problem suggested by a sentence in the opening chapter of 'The Warden' by Anthony Trollope. The hero of the book, Mr. Harding, was Warden of a legacy to twelve old men, and "being an open-handed, just-minded man, . . . he had . . . declared his intention of adding twopence a day to each man's pittance, making a sum of sixty-two pounds eleven shillings and fourpence, which he was to pay out of his own pocket." Now it is clear that there is some numerical slip here, for 2*s.* a day (*i. e.* 2*d.* a day to each of 12 men) amounts to about £36 10*s.* a year. The question is, what was the nature of the slip which leads to the above result? Astronomers have to deal constantly with errors of record, and should be specially good at solving a conundrum like this.

From Tolstoi's 'Anna Karénina,' Chapter 15:—

Venus, with silvery light, shone out in the west; and in the east, Arcturus gleamed, with his sombre, reddish fire ...... but Levin resolved to wait until Venus, which was visible through the branches of his birch-tree, rose clear above the hills on the horizon.

From a review of some book of reminiscences, reference mislaid:—

Mr. Locker-Lampson said Hallé's cook once played on the number 22 and won a prize. "How did you come," said her master, "to light on the number 22?" "In this way," was the answer; "I dreamt three times of the number 7, and as three times 7 is 22 I played on 22." Locker-Lampson told this story once to Arthur Stanley, the least arithmetical of reasoning creatures. He looked very blank, but at last said: "I suppose three times 7 is not 22!"

The death of Jules Verne at Amiens on March 24 should not pass unnoticed by us. From notices in the daily press we learn that they were very proud of him at Amiens, where he lived a quiet almost sedentary life, only once interrupted by a few journeys to the English Channel and the Mediterranean in a small yacht which he purchased but was soon tired of. His fifty odd volumes have been translated into almost every language, including Japanese and Arabic; and will probably long delight the youth of all nations. The reckless ingenuity with which he escaped from the fetters that bind Science as we know it make his writings a fund of amusement of a unique kind: indeed those of us whose imagination is not very well developed may even find them a source of profit; though not many of us will be able to follow one eulogist who dubs Jules Verne "a man probably as great in moral purpose and effect as he who preached the first Crusade."

One of Jules Verne's earliest successes was a short story called a "Drama in the Air," dealing with a madman who got into a car of a balloon, and when in the skies tried to kill his companion. There have been thrilling balloon stories written since then; and I picked up one of them on a railway bookstall the other day, called "A Race with the Sun" by Mrs. L. T. Meade; with an exciting picture on the back, of a man clinging to a car-less balloon and an explosion happening beneath him. It appears that he was tied up to a plank attached to the balloon, together with a bottle of hydrogen and chlorine mixed, and some nitroglycerine; but he managed to untie himself and get rid of his dangerous companions, which fell to earth and exploded at a safe distance—assuming there was nobody below. The part played by the Sun in the matter is perhaps best described in the words of the gifted authoress:—

> "Hydrogen and chlorine," I muttered to myself. "The dumb-bell shaped glass vessel which is fastened under the plank contains, without doubt, these two gases, and the tin canister which rests beneath them is full of nitroglycerine." Yes, I knew what this combination meant. *When the first glint of the sun's rays struck upon the glass vessel it would be instantly shattered. The nitroglycerine would explode by the concussion, and the balloon and myself would be blown into impalpable dust beyond sight or sound of the earth.*

When it is added that dawn was already appearing, it will be realized that the hero's position was by no means free from anxiety.

Answer to Conundrum number 1: *Nostoc* is a "genus of Algæ found in moist places, *Witches' butter*, *Spittle of the Stars*, *Star-jelly*."

Answer to Conundrum number 2. This can only be given by conjecture, but since 24 shillings a week is £62 8*s.* 0*d.* for 52 weeks, and the 3*s.* 4*d.* required to make up the total named is one-sixth of £1, it seems as though Trollope had reckoned the year as consisting of $52\frac{1}{6}$ weeks of six days each; had taken 2*s.* a day as equal to 24*s.* a week instead of 12*s.*; and to allow for the fraction, had taken one-sixth of £1. But this is a terrible "derangement" of blunders; and I am far from satisfied with it, though it is the best I can do. Perhaps another solver may be more fortunate.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVIII. JULY, 1905. No. 359.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1905 June 9.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: E. T. WHITTAKER, M.A., and
T. LEWIS.

THE Minutes of the last Meeting were read and confirmed.

Mr. Lewis. There have been 68 presents received since the last Meeting. Calling for special attention is an ancient volume presented by Mr. Knobel, entitled 'Commentatio de ortibus et occasibus Siderum apud Classicos commemoratis,' by J. F. Plaff. Also there may be mentioned 20 enlargements of the Astrographic Chart negatives, in continuation of those previously received from the Royal Observatory, Greenwich, and a publication of the West Hendon House Observatory, Sunderland, presented by Mr. T. W. Backhouse.

A vote of thanks was accorded to the donors of the presents.

Mr. Cowell read his paper on the discordant values of the principal elliptic coefficient in the Moon's longitude. He showed by a lantern-slide six values as originally given which had a range of $0''\cdot76$, and the same values corrected with a range of $0''\cdot41$. The corrections producing this better accordance are of two kinds: (i) solar, (ii) planetary. The nature of the solar correction is best exhibited by an analogy. Suppose Venus observed at 6 P.M. every evening with a view to determining its rotation, and supposing it was always seen to be turning the same face to the Earth. Such an observed fact would be capable of two explanations—(α) that Venus did not rotate at all, (β) that Venus rotates in 24 hours. To assert either explanation without disproving the other would be erroneous. A precisely analogous error has been committed by both Airy and Prof. Newcomb. Because the Moon is only observed when nearly full, it is possible to attribute (and, as a matter of fact, Airy and Prof. Newcomb have so attributed) errors in two terms whose arguments are the sum and difference of the mean anomaly and mean elongation to errors in the Moon's eccentricity. The effect of planetary terms is to produce a wave in the observed values of the eccentricity. One of these waves is 37 years in length, and therefore discussions such as those of Prof. Newcomb and Mr. Nevill, based on 12, 13, and 16 years, are liable to large corrections, if the years included in the discussion happen to include the crest or trough of the wave. The existence of such waves shows the importance of extending the discussion over as long a period as possible.

Mr. Cowell next drew attention to two terms, each producing a wave nearly nine years in period, and such that the two waves cancel each other in 1870, the approximate mean date of Prof. Newcomb's second series of observations and of Mr. Nevill's. Prof. Newcomb's results for each separate year from 1862 to 1874 consequently show no trace of these two mutually cancelling waves. But in 1850 the two waves do not cancel, and Prof. Newcomb's annual results from 1847 to 1858 exhibit fluctuations that do not appear in the later series. This, perhaps, led Prof. Newcomb to doubt the relative accuracy of the earlier series, and we find him, in his celebrated paper in which he discovered the Jupiter evection term writing: "I consider that the results for the second series are entitled to three times the weight of those of the first."

The President. I will ask you to return your thanks to Mr. Cowell for another of his interesting contributions on lunar matters, and to congratulate him on the success he has attained. As Lord Rosse is here to-night, I will take the opportunity to ask him to tell us something about his researches on lunar heat.

Lord Rosse did not present a paper formally to the Society, but gave notes on the results of experiments made at his observatory, some of which have already been published *.

Prof. Turner. Lord Rosse seems to have indicated an interesting line of work for several reasons. One advantage of it, as he has shown, is that it need not demand any elaborate apparatus. The apparatus he sketched is well within the resources of most amateurs, and therefore one might hope that this interesting work would satisfy the needs of some of those who from time to time do not quite know what work to set about †.

I am very glad to find in this another instance of the appreciation which our somewhat neglected satellite is attracting. We have the researches of Mr. Cowell and Prof. Brown in putting the lunar theory right, we are just publishing a memoir on the selenography of the surface by Mr. Saunder, and now we are having our attention drawn to the physical condition of the Moon. I am sure we have all been very much interested to see how easily and naturally the deduction as to something of the physical nature of the Moon follows from the simple fact that the light from the Moon does not pass so easily through glass as sunlight, showing that it is not simply reflected sunlight. But at one point in the paper it is suggested that a certain proportion may be directly reflected sunlight. It was stated by Lord Rosse that the amount transmitted by glass was at maximum at Full Moon, and, if I understood him rightly, he said he was not sure why that was. I respectfully suggest that if a portion of the light really is directly reflected that portion would be greatest at Full Moon, when the Sun would be shining directly on the lunar surface.

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* These notes will be presented in a revised and amplified form at some further opportunity. It is therefore thought better to defer publication.

† [After the Meeting, Mr. Saunder kindly drew my attention to the work of Langley and Very, which seems to render it doubtful whether simple apparatus can be used with advantage. With his permission, I sent the notes he made to Lord Rosse, who has expressed his obligations for them and his intention of following up the clues contained. Meanwhile, he wishes it to be

Mr. W. W. Bryant. I should like to ask how far the physical experiments referred to showed the possibility that the heat was absorbed and emitted rather than reflected from limestone or whatever the Moon's substance was assumed to be, and whether the absence of definite lag during an eclipse did not suggest that a great deal of it must be reflected. Otherwise the time of an eclipse would be expected to be later for heat-rays than for light.

Mr. Saunder. Lord Rosse did not say how he had allowed for the amount of absorption by the atmosphere, but I have no doubt but that the point has been considered. It is a very important one in this work, because the maximum emission of heat by the Moon occupies almost exactly the same place in the infra-red spectrum as the principal atmospheric absorption-band, and it occurred to me as just possible that the anomalous observation described by Lord Rosse might be connected with some change in the hygrometric state of the atmosphere. Of course, as the altitude of the Moon would make a considerable difference to the amount of atmosphere the heat had to pass through, it would make a difference in the amount absorbed.

Dr. Rambaut. I think the number of interesting points which arise out of Lord Rosse's notes show the advisability of continuing this kind of research. The result hitherto shows that it would be a great pity if the observations were not continued and not pushed as far as they can be. There is one question I would ask Lord Rosse about. If I recollect aright—but I speak only from recollection of his published paper on the heat of the Moon—in the eclipse of 1888 the temperature did not return to the same

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understood that he had no intention of discussing the present state of our knowledge—he was merely summarizing the work at Birr Castle. Speaking entirely on my own behalf, I fear there is little excuse for me in not having the work of Langley and Very more definitely present to my mind, for there is, I find, an important reference to it in this magazine so recently as March (see p. 136).—H. H. T.]

height as at the beginning of the eclipse; but would that not be what one might expect? If the heat from the Sun is rapidly radiating away, as soon as that supply of heat from the Sun is cut off the temperature would very rapidly diminish, and although the supply was the same after the eclipse was over it would have considerable leeway to make up, and one would expect it would be some time after the eclipse was over before the lunar heat became normal and the condition prior to the eclipse would have returned. In fact, we should have it fall very much during the duration of an hour and a half, so I think it is hardly a matter of surprise that the temperature at the end should be lower than at the beginning.

The President. I am sure we shall all agree with Prof. Turner that this investigation should be carried on further. We are very glad Lord Rosse has brought the matter forward, and we hope that, as Prof. Turner has hinted, the matter may be taken up by other observers. The results of observing the two limbs of the Moon during, say, the first or third quarter would be of considerable interest if it could be done, or if it were possible to observe the temperature of small areas of the Moon situated on opposite sides of the terminator, some interesting information might be gained, as with a morning terminator the temperature of an illuminated area could be directly compared with that of a dark area which might be expected to have attained its minimum temperature. I should like to ask Lord Rosse if he would kindly communicate to us any further results he may obtain, for I am sure we shall all be glad to hear them.

Mr. Dyson described a paper from the Royal Observatory, Greenwich, on the diurnal variations of the nadir-point and of the level of the Greenwich transit-circle. It was found by Mr. Thackeray some years ago that the observations of the nadir and level about 6 o'clock in the evening gave results which differed from those made near noon and midnight, and Prof. Turner suggested that observations should be made daily about 6 o'clock in the morning to see whether the daily fluctuation was of single or double period. It was not found necessary to make special observations, as sufficient are obtained each year to get a good idea of the change of level. Taking the level error of the transit-circle as zero at 6 P.M., at midnight it would be $+0''{\cdot}22$, at 6 A.M. it would be $+0''{\cdot}38$, and at midday $+''0{\cdot}26$. The observations of the nadir did not show nearly so much variation. It is presumed that these changes are due to diurnal movement of the piers which support the instrument.

Prof. Turner. I should like to thank Mr. Dyson for the paper he has just given us. The circumstances he referred to were these. In the Greenwich reports of the last few years these particulars were missing, for the simple reason that 6 o'clock in the morning is not a pleasant time to make observations of any kind, but for a particular purpose it seemed advisable to ask for them. It was clearly interesting to known whether these values of level at that hour would be the same as they were at 6 in the evening or whether the variation was diurnal. If I might offer an opinion, I think it is the hill which twists about. My attention was drawn to this by Prof. Milne, who was investigating the matter and wanted to know whether there would not be diurnal changes due to the variation of sunlight. I do not think that at Oxford, where the transit-circle is on a flat plane, we get such inconsistencies, and it is interesting to know whether the ground of a flat plane moves as compared with that of a hill.

Mr. Thackeray. I believe when a paper of the kind was read before the Society some years ago Prof. Darwin said that from observations made at Cambridge he could not attribute such a diurnal effect as this to a tidal cause.

Dr. Rambaut. Has Mr. Dyson compared these figures with the number of people on the hill?

Dr. H. N. Russell. Some years ago the United States sent an astronomical expedition to Chili, and it was found that the diurnal variation there was so great that when they got back the observations were valueless.

Mr. Franklin-Adams. I should like to tell an interesting story of the diurnal change which an astronomical friend of mine found at his observatory. The level error of his instrument varied throughout the day, first by several seconds of arc, then by several seconds of time, and finally by minutes. Then the whole thing was brought down and it was found that the energetic builder to strengthen the pier had put in it a large piece of gas-barrel, so that when the Sun shone on the gas-barrel side the change was enormous.

The President. I should like to ask Mr. Dyson if there appears to be any connection between these variations and diurnal variations of meteorological conditions. Has, for instance, the variation been more in bright weather or has it been less?

Mr. Dyson. No, I do not think so. It would be very difficult to disentangle anything of that kind, but no doubt it could be done if the subject were worth the trouble. It is only interesting to know what is the result as it affects the observations.

Mr. Gheury. I have heard a paper read in Paris in which a similar variation to this was proved by calculation, but I cannot tell how far the theory was accurate. The variations were supposed to be in exact accordance with lunar time, and there was a small tilt of the vertical due to minute undulation going through the surface of the Earth, just as there is when you drop a stone into the water. There may be something of that description acting in this case.

Mr. Inwards. I remember Airy contributing something of this kind to the Society and it was received with considerable doubt. It is interesting to know that perhaps after all he was right.

Mr. Hinks (reading a paper announced at the meeting in May on "The Determination of Stellar Motions without Reference to Meridian Places"). The time has come when perhaps we may invert our ideas as to the best way of determining proper motions of stars, and instead of working backwards from what we may call the foreground of brighter and, on the average, nearer and faster moving stars, we may by the application of photography work forwards from what we may call the background of fainter and more distant stars.

There are in the present method of determining proper motions a number of well-known difficulties. There is one to which I should particularly like to call attention—namely, the probability that the constant of precession, determined, as it is, from a limited number of the brighter stars, is not applicable to the stars in general. This point was raised by Prof. Kapteyn in his address delivered at the St. Louis Congress and by Mr. Dyson and Mr. Thackeray in a paper they lately presented to the Society. They were discussing the position of the solar apex and they found that a different result was arrived at according as they used bright stars or faint ones; and the question is, whether we can take this as being due to motion of the background. The objection may be raised that there may be no motion of the background. It is possible in the last resort to get over the difficulty by the use of meridian places, but by the use of photographic methods one gets over the difficulties without reference to continually shifting planes which are inseparably bound up with meridian work. I therefore make the suggestion that we should try to work forward from the background and not backward from the foreground, and that this scheme should be tried by determining the proper motions of certain groups of stars in different parts of the sky. If such a scheme were worked out it would be a pity to choose the regions on any cut-and-dried plan, because then all the things of especial interest would inevitably be missed. But the Astrographic Catalogue is now getting to a respectable age, and a great deal would be gained by using some of these plates now twenty years old. We hope to do something in this connection at Cambridge shortly. Meanwhile I shall be grateful for any remarks that may be made on the general idea, because it seems to me rather an interesting sort of paradox to say that the only way to get proper motions is to use only relative measures.

Mr. Dyson. Having been so directly challenged in Mr. Hinks's opening sentences, I must reply at once. I do not think Mr. Hinks quoted my words exactly*. It is of slight importance whether rectangular or spherical co-ordinates are used, but it is of importance whether positions and proper motions employed are relative or absolute. By measures of photographs of the same field taken some years apart, such as Mr. Hinks proposes to make, relative motions of the stars on his plates will be determined, but not the mean movement of all the stars on the plate. For a comparison of the movements of stars in one part of the sky with those in another, absolute proper motions are required. So far the transit-circle has been the instrument with which these have been determined, and the results of the observations largely due to the discussions of Auwers and Boss have given us a general knowledge of the movements and mean distances of the stars down to the 9th magnitude.

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* The sentence referred to by Mr. Hinks reads (see *Observatory*, March 1904, p. 131, line 6):—"If the photographic method is to be limited 'to obtaining the *relative* positions of objects on the plate' and 'the avoidance as far as possible of the use of meridian places,' I do not think it will get very far with the one exception of work on stellar parallax."

One very interesting result will be derived from Mr. Hinks's measures. Among the stars brighter than 9·5 magnitude on the B.D. scale there are approximately 100 moving 1″ a year and 10,000 moving 10″ a century, or approximately one star in each 4 square degrees. By measures of plates 10 years apart, Mr. Hinks will obtain the valuable result of what percentage of faint stars, say 15th to 16th magnitude, have these large proper motions. There may be 1 per 1000, 1 per 500, or 1 per 100—we are in entire ignorance on the subject.

It seems to me that this will be an extremely valuable piece of work, but when it comes to sorting out the residuals and inferring the proper motions of groups of stars there will be great difficulties and, I fear, a danger of moving in a circle.

Prof. H. H. Turner. I think in his anxiety to assert his independence Mr. Hinks has perhaps been needlessly antagonistic. It was not necessary to his point that he should use this method *instead of* the old meridian methods; but it is important to point out, and I entirely agree, that they may be used *in addition to* these methods; and certainly it should be tried as a possible check upon them. In that way his suggestion is most valuable; and I look forward very hopefully to the results obtained in this way.

First of all I would ask permission to make a few numerical remarks. It seems to me important in a question of this kind to get some notion how many stars there are of the kind we are talking about. Mr. Hinks was kind enough to send me a proof of his paper, and I saw there was going to be a discussion, but there was not much time to prepare. However, on looking over our plates I find we had two plates taken at an interval of ten years apart of the same region. In course of looking over the results we found on the first plate only 339 stars, which was not enough and so a second plate was taken. All the measures of the first plate were compared with the measures of the second. The measures were of the character that we have been making for the Astrographic Catalogue, the unit being a third of a second of arc. The following Table shows in a diagrammatic manner the differences which are found between the two plates:—

Crude differences after 11 years (4 stars with large P.M. omitted).

Δy									
$+1''\cdot 2$	0	0	0	0	3	2	2	0	0
$+0\cdot 9$	0	1	1	3	4	1	0	3	0
$+0\cdot 6$	1	1	3	6	14	9	2	1	1
$+0\cdot 3$	0	2	5	12	14	12	10	2	2
$0\cdot 0$	1	3	7	17	28	10	10	4	1
$-0\cdot 3$	0	2	7	16	12	16	3	3	3
$-0\cdot 6$	1	2	4	8	8	16	4	1	1
$-0\cdot 9$	0	4	2	2	2	2	4	1	0
$-1\cdot 2$	1	2	2	2	1	4			
$\Delta x =$	$-1''\cdot 2$	$-0''\cdot 9$	$-0''\cdot 6$	$-0''\cdot 3$	$0\cdot 0$	$+0''\cdot 3$	$+0''\cdot 6$	$+0''\cdot 9$	$+1''\cdot 2$

There are 28 stars which show no change at all to the unit adopted; and 109 more which show no change greater than one unit in x or y. Now there is *à priori* no reason why these numbers should be so large: that large numbers of stars fit accurately on to the same places after an interval is good evidence for the existence of very remote stars, or, as Mr. Hinks calls it, a background to which the movements of others can be referred.

For consider what the other alternative is. The alternative is that these stars are actually being carried along together, and you must suppose that they are all at the same distance, for otherwise the parallactic motion of the Sun itself would separate them; if not they must be the same distance and moving along together; and it seems to me that, at present at any rate, this alternative must be put aside. And this large number of stars which fit after eleven years is to be further increased when we make allowance for the accidental errors of measurement and reduction, which themselves tend to introduce differences of a spurious kind between the plates. The spreading out of the stars is, in fact, largely accidental, and we may make allowance for that in rather a simple way. The spreading out, assuming that it was real, would of course increase in course of time, and after a certain lapse of time we should find the spurious effect of accidental error represented as a real spreading out; so that we can make allowance for the accidental error in this Table by multiplying the 11 years by some factor; and the only thing is, how much we should multiply by—after how long will this fictitious representation of spreading out become a real one? Well, that is a difficult question to answer exactly. I can certainly give an inferior limit, because, comparing two actual measures of the same plate, we get the probable error of a measure of the same image, which shows that as an inferior limit we must *double* the 11 years, and by rough calculations, which I need not here specify, I should be inclined to make the factor four times. I fancy that after 30 or 40 years we should get about the scattering represented in the table, and hence there would be a considerable number which show less than the third of a second displacement after 30 or 40 years. I may say, with regard to the point raised by Mr. Dyson, there are four stars having quite definite proper motions, the largest of them has a proper motion of 17 units in the 11 years—not a very large one.

Further, it seems to me we have another test. We cannot apply it at this portion of the sky, because it happens to be near the apex of the Sun's motion. But suppose we were at a distance from the solar apex, where the drift of the stars is considerable. Now that drift depends for a given star upon its distance from our system, and will roughly vary with its own proper motion, which also depends upon its distance from our system, and will roughly vary with its own proper motion, which also depends upon its distance from our system. So the drift will be roughly proportional to the proper motion. Supposing such to be the case, what will happen to the distribution of positive and negative motions? Instead of the double motion in x and y, take the distribution in one co-ordinate only. We may suppose the actual distribution symmetrical. There are a large number so remote that they are not apparently moving, and an equal number moving one way or another. But the drift due to the Sun's motion bends the curve on both sides in *the same* direction; and where the proper motion of the stars is large they are probably on the whole nearer to us and the parallactic drift is large. Nearer the mean the parallactic drift is quite small, and the mean itself will not be sensibly disturbed. (I am exaggerating a little because there is one reason why the mean *is* disturbed.) The general result is we should get a curve (screen) which is no longer symmetrical, but steeper on one side than the other. It seems to me that this might be tested in regions far away from the Sun's apex, but we should want accurate measures to make the test possible. The rough measures afforded by the Astrographic Catalogue would not be good enough for many years. By taking two plates which may be superimposed and the distances measured accurately, I have every reason to believe that we should be able to apply this test after a small interval; and I would attach much importance to it as a test whether our position was correct.

Mr. H. N. Russell. I also have had the privilege of reading Mr. Hinks's conclusions before they have been submitted to the Society, and I certainly agree with him that photography is likely to give better results of this kind than transit-circle work. We have not so much accidental error to deal with in photography, and I am inclined to think that when the thing is properly worked out the way suggested for determining proper motion will be better than the way now used.

Dr. A. A. Rambaut. I think we must all have admired the lucidity with which Mr. Hinks explained this problem to us; and we cannot fail to see the great importance of the subject. Mr. Hinks introduced the subject of rectangular co-ordinates as if apparently he expected to be met with considerable opposition. But I think the feeling at the present day shows there is no great objection to rectangular co-ordination, and the jealous feeling which seemed to mark the older school of observers has quite disappeared. At the same time I should like to ask Mr. Hinks how his proposition differs from that of Professor Kapteyn. His scheme in the main seems to me to do very largely what Mr. Hinks now proposes. Of course there are some peculiar features about it. He proposes to photograph the same field of stars three times over at the maximum of parallax in that particular region at intervals of three months, not developing the plate till three exposures have been obtained. That is a detail, for there is nothing to prevent his plan being carried out by developing the plates in the ordinary way. Professor Kapteyn's plates when they have been measured and examined, and when the results from them are treated according to his plan, will give the proper motion and the parallax referred to the background of which Mr. Hinks now speaks of. It is true that, in the first place, we only get the proper motion from a single year interval; but it is also a part of Professor Kapteyn's proposal to repeat these observations at intervals of time so as to strengthen the proper motion determination. It seems to me we have here the very idea of referring the proper motion of stars to this practically immovable background.

Mr. Dyson. I really think the expression of practically immovable stars ought to be used with caution. We know absolutely nothing of stars of magnitude 15 or 16. We know that stars of the 9th magnitude are in the mean a little and a very little further than those of the 8th magnitude, and these again are a little, but only a little, further off than those of the 7th magnitude, and so on; but about the actual distances of fainter stars we know nothing except by extrapolation from these results. If the planetary system may be taken as an analogy, there are hundreds of bodies nearer than Jupiter. There are hundreds of stars nearer than Arcturus. The idea that faint and slow-moving stars may be taken as an immovable background must be taken very cautiously. For my part, I agree with the Greek philosopher who said πάντα ῥέι: "They are all moving."

Dr. Rambaut. I do not think we need assume a background of immovable stars. That will take care of itself. If the plates taken in this way are reduced we should be able to detect the proper motions. By solving equations of condition for proper motion and parallax we at once detect those that have a proper motion; we can then reject them and so go on to what is, if not an immovable background, at all events a background in which all movements are alike.

Mr. A. R. Hinks. I am much obliged for the benefit of the criticisms offered. In the first place, I must earnestly disclaim any intention of being antagonistic. Prof. Turner said he thought I had been needlessly antagonistic, and possibly I may have expressed myself badly. The idea is this: at the present time we are proposing to get the proper motions of the fainter stars from photographs, and the question is whether these will agree with the proper motions already determined by meridian methods. My idea is there would be advantages in going round and seeing if we met the meridian results or not. With regard to Dr. Rambaut's question as to how this plan differs from Prof. Kapteyn's, I have had the pleasure of reading a scheme which has been circulated by Prof. Kapteyn, and, as far as I understood him, he proposes to hang his photographic places on meridian places. He does not propose to go round the other way, so I think that that is a distinct point of difference between what I propose and Prof. Kapteyn's scheme. So far as the question of background is concerned, I said that what we would at present call the background is simply a convenient word. I do not wish to insist that it is immovable, but I do think we shall find that at least we can pick out a background which is immovable within the definition of immovability that we can give by any observations at all that we can make. I do believe that will be possible. Finally, I should like to thank Prof. Turner for having at short notice brought forward his very interesting numerical examples from the Oxford plates.

Mr. Whittaker gave the substance of *Mr. Denning's* paper on "The Meteors from Biela's Comet." Last November a well-marked return of the Andromedid meteors had been observed by Mr. Ellison, of Enniscorthy, Ireland, and others, and Mr. Denning discussed these observations. It appeared that owing to planetary perturbations the date of the shower was being gradually moved forward, and was now at about the same date as the Leonids. This shower might be expected to supply information as to the connection between meteor-showers and comets, such as the Lyrids and Perseids, which represented more ancient systems. The Andromedids are now becoming of the same type as these. Mr. Denning anticipated that this would become an annual shower and that there would perhaps be a brilliant display on November 18 next at 10 o'clock, which would be continued for several nights.

Dr. Downing. Quite opportunely I have finished a piece of work on which I have been engaged for a good many months, on these meteors, so I can bring forward my notes as a commentary on Mr. Denning's interesting paper. In my calculation we ought to meet the central portion of the swarm next November. The radiant I have adopted is deduced from the observations of the shower of November 1885, and from that, and with a period of $6\frac{2}{3}$ years, I have computed the elements of an instantaneous ellipse, and also the perturbation of these elements by Jupiter and Saturn during an interval of 20 years, which are exhibited in the Table now on the screen. In this orbit the aphelion distance is 6·2, so that the meteor-stream crosses the orbit of Jupiter and extends beyond it to about the unit of distance. It does not, however, reach the orbit of Saturn, whose mean distance is 9·5. During the twenty years there were conjunctions with Jupiter in

1890 August and in 1901 February, the effects of which will be noticed in the Table. The meteor-stream meets the Earth at the descending node of its orbit, and the Earth will be in this longitude on 1905 November 18 at 10^h G.M.T., but it appears that the segment of the meteor-stream to which these calculations refer will pass through the descending node on October 16. In fact it appears that the orbit has become contracted in the twenty years' interval; the periodic time has altered from 6·667 to 6·612.

As the general result of these calculations, therefore, it appears that the most probable date for the centre of a shower of Bielid meteors this year is November 18. If there be a shower on that date it will indicate that the meteor-stream is in this part of sufficient length to occupy at least 33 days (October 16–November 18) in passing a definite point of its orbit, or that there is another swarm following the main swarm at this interval, and is also of sufficient extent in the direction Sun–Earth to allow of some of the meteors encountering the Earth, although the centre of the stream is more than a million miles outside the Earth's orbit at the time.

Prof. A. S. Herschel. We must all congratulate Dr. Downing very heartily on having completed this laborious calculation in such abundantly good time for the expected return this year of the Biela's Comet meteor-shower, so that the conclusions arrived at by theory may, if possible, be verified by observation. The results appear to give rather a meagre prospect of a bright display this year, for one main reason at least, which may be found to be detrimental to the shower's abundance, that the perturbed path of the swarm passes, it appears, fully a million miles outside the Earth's orbit; for it will be remembered that the disappointment which recently awaited expectations of the Leonid shower's copious appearance without doubt arose from a displacement, by the effect of perturbations, of the Leonid cluster's orbit to somewhat more than a million miles inside the Earth's orbit. But the large shift of the nodal date of the shower's recurrence to November 18th in the present year is at least a feature of the Andromeda or Bielid meteor-shower's approaching return to be looked for and expected, which we should not have been able to anticipate without the careful calculation which Dr. Downing has now brought so successfully to a satisfactory conclusion.

Mr. Hinks described a paper by *Mr. H. N. Russell* and himself on the determinations of stellar parallax from photographs taken at the Cambridge Observatory. Two years ago, Mr. Russell and I were doing some parallax work at Cambridge, and at that time planned a scheme, which, owing to Mr. Russell being appointed a research assistant of the Carnegie Institution, was put into operation. Mr. Russell is going back to America shortly, but I am glad to say that the work will not stop on that account, for we hope to go on providing plates at Cambridge, and a large share of the work of measurement and reduction will be done by Mr. Russell after he has got back to America. But I am exceedingly sorry to lose such an energetic colleague in the work that we have in hand. With regard to the general scheme adopted, we were not able to follow out Prof. Kapteyn's idea of accumulating exposures at successive epochs, upon the same plate, and we were fully convinced that all the plates should be taken on the meridian. We desired to keep two objects in view—namely, (1) to secure the most advantageous relation between the amount of labour to be expended and the probable accuracy of the results to be achieved, and (2) to eliminate all known sources of systematic error. Under the circumstances we decided to take separate plates at each epoch and develop them at once. Each plate was to have four exposures. Many of the stars which promised to give most interesting results were too bright to give measurable images on a plate exposed for three minutes, and we therefore decided to use a special form of colour-screen. The serious difficulty with a colour-screen is that it is not permanent, and it was realized that it was necessary to form a screen which was permanent, one which was not easily scratched and which could be cleaned, and experiments were now in progress with a view to attaining that end. The most serious of the various systematic errors found in the plates were those which might be included in the category of hour-angle error, the two principal causes of which might be optical distortion, varying with the hour-angle, or atmospheric dispersion. To secure freedom from these errors we decided to work at a definite hour-angle for each star, and the decision arrived at was that all photographs should be taken within half an hour of the meridian. The observations connected with one part of the paper are well advanced, and it is hoped they will be published within the next year or so. In conclusion, I suggest that astronomers interested in the determination of stellar parallax should observe a few stars in common, regularly, and I should like to add that both my colleague and myself will welcome any suggestions or criticisms.

Mr. Russell made some remarks on the paper presented by him, and, expressing his appreciation of his reception in England, thanked all his friends at the Cambridge Observatory for the assistance and co-operation which had been rendered to him while he had been there.

The President said that he was very glad indeed to know that Mr. Russell was going to continue the excellent work that he had been carrying on at Cambridge in conjunction with Mr. Hinks, and that his return to America would not prevent him continuing the work. He trusted that Mr. Russell would favour the Society with further contributions, and he congratulated him upon the excellent way in which he had condensed his present paper.

The following papers were announced and partly read :—

W. F. Denning. "The Meteors from Biela's Comet."

H. H. Turner. "On the Formula for connecting Diameters of Photographic Images with Stellar Magnitude."

P. H. Cowell. "The Moon's observed Latitude, 1847–1901."

P. H. Cowell. "On the Discordant Values of the principal Elliptic Coefficients in the Moon's Longitude."

A. R. Hinks and H. N. Russell. "Determinations of Stellar Parallax from Photographs taken at the Cambridge Observatory." Introductory Paper.

S. A. Saunder. "The most probable Position of a Point determined from the Intersections of Three Straight Lines."

A. Stanley Williams. "On the relative Efficiency of different Methods of determining Longitude on Jupiter."

E. W. Maunder and A. S. D. Maunder. "The Solar Rotation-Period from Greenwich Sun-spot Measures, 1878–1901."

Royal Observatory, Greenwich. "Note on the Diurnal Variations of the Nadir and Level of the Transit-Circle." (Communicated by the *Astronomer Royal.*)

W. E. Plummer. "The Great Cluster in Hercules."

W. Ellis. "Note on the Annual Inequality in the Frequency of Magnetic Disturbance."

H. N. Russell. "The Parallax of Lalande 21185 and γ Virginis."

The following gentlemen were elected Fellows of the Society:—

Major B. F. S. Baden-Powell, 32 Prince's Gate, S.W.

Joseph Henry Elgie, 72 Grange Avenue, Leeds.

Frederick William Longbottom, Haslemere, Queen's Park, Leeds.

Frederick John Marrian Stratton, B.A., Isaac Newton Student in the University of Cambridge, Caius College, Cambridge.

John Willis, 19 Bouverie Square, Folkestone.

The following Foreign Astronomers were elected Associates of the Society:—

Th. Albrecht, Königl. Preuss. Geodätisches Institut, Berlin, Potsdam, Germany.

Gustav Müller, Astrophysikalisches Observatorium, Potsdam, Germany.

Rodolphe Radau, Membre de l'Institut, 12 Rue de Tournon, Paris.

The following Candidates were proposed for election as Fellows of the Society:—

Charles Frederick Aspinwall, B.A., Chestergate, Macclesfield (proposed by E. W. Hobson).

Hubert Hayward Champion, Master at Uppingham School, Rutland (proposed by H. H. Turner).

Lieut. Alfred Henry Laurence Ferris, R.N.R., Hughenden, Coleraine Road, Westcombe Park, S.E. (proposed by P. Groves-Showell).

Edward MacFarlane, Under Secretary for Lands and Chief Surveyor for New South Wales, Department of Lands, Sydney, Australia (proposed by T. F. Furber).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 May 31.

President: A. C. D. Crommelin, in the Chair.

Secretaries: J. A. Hardcastle, J. G. Petrie.

The Minutes of the previous Meeting were read and confirmed.

The list of presents received included Vols. 19–26 and 31–39 of the 'Vierteljahreschrift' of the 'Astronomische Gesellschaft' from Dr. Downing, to supplement Vols. 27–30, already in the library. The Rev. S. J. Johnson also presented a map of the track of the total solar eclipse of June 29, 1927.

The names of seven Candidates for Membership were read and passed for suspension, and the election by the Council of five new Members was unanimously confirmed.

Mr. W. Heath read a paper on "The Moon's Radius," in which he pointed out that in reducing occultations of stars by the Moon, account should be taken of irregularities in the Moon's limb. Tables seemed to be wanted to give an answer to the question whether, in dealing with a large group of results, the variation of the radius could be regarded as subject to the law of errors. The law would hold good if the Moon's limb was a perfect circle, but if there were high tablelands and deep basins with nearly level floors they would, if they were of large area, affect large groups of results in the same manner, and the law would not hold. He submitted a table based on the results given by Küstner and Battermann. He had made no allowance for the Moon's libration in longitude, but only considered the variation of the radius with position-angle. He hoped that at any rate a table would be formed like Struve's, giving corrections for latitude, and if corrections for longitude could be included, all the better.

Mr. Saunder said that what he was afraid of was that if libration were not allowed for the correction would be worth very little indeed. He thought the variations, taking such an extreme case as that mentioned, would be two or three times the mean amount.

The President said he was very glad that Mr. Heath was taking up the question. It was a thing that had troubled observers of the Moon at all times—the inequality of the limb. During the last few months they had been trying at Greenwich to get rid of the errors caused by the inequality of the limb in meridian observations by observing, as well as the limb, the bright crater Mösting A, which was near the centre of the disc, and they found the observations ran much more smoothly taking that than taking the limb.

A paper by *Mr. W. E. Besley* on "A Cape Colony Meteor" was read by *Mr. Hardcastle.*

Col. Burton-Brown remarked that it would be very interesting to have the analysis of the stone.

The President said that Prof. Cohen, to whom it had been sent for analysis, died on April 13. The report did not say whether or not he had completed the analysis. If not, doubtless some one else would analyze the stone.

Mr. Hardcastle read a paper on "The Priming and Lagging of the Tides," illustrated by a number of diagrams.

Mr. Lynn pointed out that there was a solar as well as a lunar tide, and though the lunar was about 2½ times as great as the solar, the latter must not be left out of consideration. As the lunar tide is greatest when the Moon is near perigee, so also the solar tide is greatest when the Sun is near perigee, which, at this stage of the world's history, is during the winter in the northern hemisphere.

The President said that immense numbers of self-registered diagrams of the actual tides had been analyzed in ways similar to those described in the paper read. The difficulty was that the wind was a factor which they could not predict, and in many places the wind had a considerable influence on the tides.

Mr. Maunder said that the Council had sanctioned the re-publication in the next number of the 'Journal' of the "Eclipse Suggestions" drawn up for the 1900 eclipse. He (Mr. Maunder) had thought he would not be going to observe the coming eclipse, but he had received an invitation from the Prime Minister of Canada to join the Canadian official expedition to Labrador, and the invitation included Mrs. Maunder. The Astronomer Royal had approved his accepting this most gratifying invitation. This circumstance had compelled him to resign his position as Secretary of the Eclipse Committee, which position, he was glad to say, had been filled by the appointment of Mr. Thwaites.

Mr. Thwaites said that a letter had been received from one of the Astronomers of the Spanish Government, intimating that a decree had been passed by the Government offering free entry for instruments and apparatus in connection with the eclipse, and asking that the names of Members who were intending to go should be forwarded to him, together with the places they intended to occupy, in order that he might notify his Government, and that the Customs authorities might be able to make arrangements for free entry.

A Member inquired whether this arrangement would apply to small instruments carried as luggage.

Mr. Thwaites said the communication mainly applied to bulky instruments. Small hand-telescopes, opera-glasses, and cameras, &c. would be admitted free if the names of the owners were known to the Spanish Customs.

Col. Burton-Brown read a paper on the most suitable stations from whence the total eclipse could be most advantageously viewed. As the result of a personal visit, the writer recommended Pola de Lena (near Oviedo), Torreblanca, Oropesa, the Columbretes Rocks, Pta. de Serra (Ibiza Isle), Cabrera Isle (Mallorca), Bougaroun, Collo Harbour, Robertville (near Philippeville), and Djidjelli (north of Africa) as the most suitable places for observation. He had purposely avoided mid-Spain stations, which were well known and would be fully occupied. Col. Burton-Brown concluded by strongly recommending the chartering of a steamer to call at the various stations for the purpose of taking out and bringing back observers.

Mr. G. F. Chambers said he entirely concurred in Col. Burton-Brown's suggestion that the ideal way of seeing this eclipse would be to go by sea all the way from England, calling at various ports on the way out and home, and so having a very pleasant sort of circular trip, but he was sorry to say that the attainment of that aim seemed to be totally out of the question. It appeared, however, that the weather prospects in the north of Spain were not so unfavourable as had been at first supposed, and the harbour of Gijón afforded a safe anchorage, so that in that direction there seemed the possibility of a short, convenient, and comparatively inexpensive trip. The Committee proposed to make further inquiries, and might be in the position to make a statement at the June Meeting of the Association.

The President said that Mr. Stromeyer had sent in a paper containing some suggestions as to utilizing the eclipse for the purpose of improving our knowledge of the form of the Earth. The idea of the writer was that some observers should be near the limits of totality, and should try to find out as accurately as possible the points where the exact edge of the shadow passed over the Earth's surface. Prof. Turner had written to him stating that he was going out to Egypt, where he would have soldiers placed at his disposal. These soldiers he would distribute in a line north and south near where the edge of the shadow would come, at intervals of 40 or 50 yards between them, and his hope was that he would find one soldier just inside totality and the next just outside, and if he could do that it would fix the edge of the shadow within a few yards. There was little doubt that if a number of observations of that kind could be obtained, they would give a great deal of information, both as to the position and diameter of the Moon, and also the size and shape of the Earth.

A note upon "The Solar Eclipse, 1905 March 6th," by *Mr. W. F. Gale*, and another on the same subject by *Mr. A. W. Dobbie*, were then read, the latter being accompanied by a number of photographs taken in an 18-inch reflector of the writer's own construction.

A note by the *Rev. S. J. Johnson* on "Mercury seen in April with the Naked Eye" was next read.

The President said that a number of little notes had come in relating to this naked-eye visibility of Mercury in April. It was evidently more conspicuous then than usual. It was worth noting that in the coming eclipse, as in 1900, the planet Mercury would be a conspicuous object during totality, not far from the corona.

Mr. Keatley Moore asked how many diameters the planet would be away from the Sun.

The President said he had not the exact figures, but it would be rather further than in 1900.

Mrs. Maunder. About eight diameters.

The Meeting stood adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

The final Meeting of the present Session was held on Wednesday afternoon, June 21, at the Society's rooms, 70 Victoria Street, Westminster, *Mr. Richard Bentley, F.S.A.*, President, in the Chair.

Mr. G. E. Simpson read a paper on the "Normal Electrical Phenomena of the Atmosphere." In no branch of physics has the discovery of "ions," "electrons," and "radio-activity" produced a greater revolution than in that devoted to atmospheric electricity. In this paper the author endeavoured to state the chief line along which during the last few years investigations have been made and the conclusions arrived at, and also to point out some of the problems awaiting solution. The amount of radio-active emanation in the lower regions of the atmosphere is increased by all those meteorological conditions which tend to keep the air stagnant over the Earth's surface. The meteorological conditions which either cause or often accompany stagnant air are calm low temperature and high relative humidity, while, on the contrary, high winds, high temperature, and low humidity generally accompany the mixing of large masses of air. This all agrees with the observed facts that the atmospheric radio-activity increases with falling temperature, rising humidity, and increasing wind-strength. The author is of opinion that a solution of the problems of atmospheric electricity can only be expected from the results of extended measurements in the atmosphere itself and from laboratory experiments directed towards the problem.

A paper by *Mr. G. P. Ferguson,* "Two New Meteorological Instruments," was, in the absence of the author, read by the *Secretary.* The instruments described were (1) "Automatic Polar Star Light Recorder," for recording the amount of cloudiness at night; and (2) "The Ombroscope," an instrument for determining the time and duration of rain. Both of these instruments are in use at the Blue Hill Observatory, Mass., U.S.A.

Longitude across the Pacific.

THE laying of the telegraph-cable across the Pacific—or rather cables, for there are two, one the property of the Commercial Pacific Cable Company, from San Francisco to Manila, in the Philippine Islands, the other the State-owned cable from Vancouver to Australia and New Zealand—brought with it a possibility that had not before existed, for it became then possible to measure telegraphically arcs of longitude which would complete the circuit of the globe. The opportunity did not lie long unheeded. In fact before the completion of the first-mentioned cable the U.S. Coast and Geodetic Survey had approached the Commercial Pacific Company, and had been granted all facilities for the work, and almost immediately the other cable was completed officers of the Canadian Survey took advantage of the existence of this connection to determine the difference of longitude between Canada and Australia.

The U.S. Coast Survey have completed and published their work, which publication is the cause of this note being written. The cable joining San Francisco to Manila consists of four links divided by transmitting-stations at Honolulu, Midway Island, and Guam Island, and time-determinations were made at each of these five places. It was at first proposed to eliminate Midway Island as a longitude station, and to join the two ends of the cable there from Honolulu and Guam, but it was found impracticable to send signals through this length of cable—about 4000 knots, so Midway Island, though difficult of access, had to form a longitude station, and the length in time of each link was determined.

Without giving all the details of the operations, it may be sufficient to say that the time-determinations were made with ordinary reversible transits of 3 inches aperture, and that though the attachment of registering micrometers to these was considered, their use was found impracticable. The observers, Messrs. Edwin Smith and Fremont Morse, exchanged stations on the arc San Francisco—Honolulu, so that personal equation was here eliminated; but on the other arcs there was no interchange, and a difference of personal equation $+0^{s}\cdot071$ was applied, derived from the above arc and a series of special observations. As the observations at Guam were made entirely by Smith, and those at Midway entirely by Morse, it will be seen that error from personal equation—assuming that neither observer changed his habit in this respect—is eliminated from the final longitude of Guam, and that the difference of personality of the two observers only enters once into the final result. If the observers had not changed stations on the eastern link, personality would not have entered at all into the chain. The following Table summarizes the work:—

	Length of cable in knots.	Transmission time.	No. of nights.	Difference of Longitude.	Prob. error.
		s		h m s	s
San Francisco Honolulu	2276	0·189	11	2 21 38·923	±0·008
Honolulu Midway	1332	0·118	8	1 18 3·220	±0·015
Midway Guam	2607	0·185	10	2 31 53·582	±0·010
Guam Manila	1631	0·156	5	1 34 43·264	±0·010
				7 46 18·989	

With the total difference of longitude found by the summation of column 5 above, the longitude round the Earth can be found, first premising that as the transit at San Francisco was placed on a pier 0^{s}·004 east of the longitude station of the U.S. Survey, and at Manila the instrument was 0^{s}·224 west of the centre of the Cathedral Dome, which is the point of reference for astronomic work there, the above result must be corrected by subtracting 0^{s}·228 to compare with the results of other arcs.

	h	m	s
Greenwich to San Francisco	8	9	48·813
San Francisco to Manila Cathedral.........	7	46	18·761
Manila Cathedral to Madras	2	42	53·000
Madras to Greenwich	5	20	59·600
	24	0	0·174

The longitude of San Francisco in the first line is derived from a compensation of four trans-Atlantic longitudes and the network of the U.S. Survey across the North American Continent. The arc Manila to Madras was determined in four links by officers of the U.S. Navy in 1881 and 1882, and the longitude of Madras east of Greenwich is that adopted for the Indian Survey in 1900, and depends largely on the longitude Greenwich—Karachi determined by two officers of the Survey in 1895, but there is certainly some little uncertainty as to the connection of Karachi with Madras. In the publication now before us Mr. Edwin Smith prefers to adopt 5^{h} 20^{m} 59^{s}·42 as the longitude of Madras, which naturally makes the circuit-error much smaller.

The last sentence written indicates that other combinations can be made to circle the Earth, and comparison of such are interesting. Here is one *via* Vladivostock, westward arcs being reckoned positive:—

	h	m	s
Greenwich to Manila (as above)	15	56	7·574
Manila to Vladivostock	− 0	43	38·500
Vladivostock to Greenwich..................	8	47	31·197
	24	0	0·271

The arc Manila to Vladivostock includes one link Hong Kong to Manila) which is used also in the arc Manila to Madras in the former circuit. The longitude of Vladivostock includes recent results (Greenwich to Poulkova) found by German observers; an earlier determination of this longitude would have made the circuit-error $0^s.103$ larger. It is known that no correction for personal equation was applied in the work of the officers of the U.S. Navy above referred to, but that it was sought to eliminate the effect of this as much as possible in the whole work by arrangement of the observers in the individual links, so that by taking parts of this, as has been done above, there is room for some error from this cause. When the results of the operations through the other Pacific cable are published, other terrestrial circuits will be available for comparison with the above. H. P. H.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Brightest Fixed Star and its Name.

Gentlemen,—

By this I do *not* mean Sirius. That star appears to us the brightest in the heavens; but as it has a parallax amounting probably to about one-third of a second, and Canopus (not much inferior to it in apparent brightness) has no appreciable parallax at all, the intrinsic brightness of the latter must be many thousand times greater than that of Sirius, and it must in fact be probably the brightest of all the fixed stars.

What is the origin of the name of this star? Canopus, in its earliest form Canobus, was the name of an ancient town situated on the mouth of the Canobic branch of the Nile, about 15 miles to the east of Alexandria, and very near the modern village of Aboukir. It has been suggested that the word is taken from that of the Egyptian god of that name. Idebr, however, quotes Aristides as saying that it is derived from two Coptic words meaning golden soil: "Kahi" = soil, and "nub" (the root-word of Nubia) = gold. He goes on to remark: "Ob und welchen Bezug der Name Canobus auf den Gott und die Stadt gleiches Namens am westlichen Ausfluss des Nils hat, ist uns unbekannt"; and adds that Manilius is wrong in affirming (I. 217) that it could not be seen until passing over the sea to the mouths of the Nile, because in the time of Hipparchus its distance from the south pole was $38\frac{1}{2}°$, and its meridian altitude was about $7\frac{1}{2}°$ at Alexandria, and Geminus remarks (c. 2) that at Rhodes it is difficult of perception unless from high ground, whereas at Alexandria it is παντελῶς ἐμφανής. But may not Manilius (if he intends to hint at the origin of the name) be right after all? To an observer looking at the southern sky from Cnidus or Rhodes, Canopus, during the short time it was above the horizon, would appear nearly over the direction of the town of that name, the best-known place on the coast of Egypt before the foundation of Alexandria.

In that excellent Cyclopædia of its day, that of Abraham Rees (still often very useful, though of course not exactly up to date), we read under Canopus, that some of the old astronomers gave this name to a star under the second bend of Eridanus. The writer goes on to say that the river in the heavens is by these taken to be the Nile, and that "this star commemorates an island made by that river, which was called by the same name." This, however, is certainly a mistake. Several terrestrial rivers appear to have been identified with the mythical Eridanus, the most usual being the Padus or Po. The Nile was contended for in Egypt, but though the boundaries of the constellation were not always taken the same, there is no authority for placing Canopus therein.

Dr. Murray, in the Oxford Dictionary, gives as the earliest use of the word in an English work, Richard Eden's 'The Decades of the Newe Worlde, or West India,' a collection of travels, some expressions in which brought upon the author an accusation of heresy. Tennyson mentions the star in his 'Dream of Fair Women,' where Cleopatra appropriately speaks of "Lamps which out-burned Canopus." Yours faithfully,

Blackheath, 1905, June 19. W. T. LYNN.

Magnitude Equation in Right Ascension.

GENTLEMEN,—

I shall be honoured if you can find space for a few words on a subject which you have noticed recently in your columns—the magnitude equation in the meridian observations of Eros fundamental stars.

We have just finished at the Cambridge Observatory an examination of the photographic places of these stars determined at eight observatories. The results from Algiers, Bordeaux, Catania, Northfield, Paris, San Fernando, and Toulouse are given in Paris Circulars Nos. 10 and 11; the Greenwich results have been communicated in advance of publication by the kindness of the Astronomer Royal.

A comparison of the photographic places from the different observatories shows that they have no sensible photographic magnitude equation relative to one another, with the exception of Algiers. The large magnitude equation in the Algiers results has already been pointed out by M. Trépied. Its cause is still under discussion, and for the present it is well to exclude these results from what follows.

If the magnitudes of the stars are fairly well distributed, the effect of magnitude equation in the meridian places is to produce a *constant* error in the deduced photographic places, but no sensible error depending on magnitude, as is well known. And it appears from the preceding paragraph that the photographic processes have not introduced any magnitude equation on their own account (except in the one case) unless the effect is the same at seven observatories, which seems unlikely. We may therefore consider that the photographic results are sensibly free from magnitude equation.

If we now compare the mean photographic places with those of Prof. Tucker's two normal systems, we have the following results:—

	Mag.	Tucker—Photographic. System I.	 System II.
		s	s
I. ……	>6·3	−0·033	−0·015
II. ……	6·3–6·9	− ·018	− ·021
III. ……	7·0–7·4	− ·017	− ·020
IV. ……	7·5–7·9	− ·011	·000
V. ……	8·0–8·4	− ·002	·000
VI. ……	8·5–8·8	+ ·004	+ ·006
VII. ……	8·9–9·2	+ ·012	+ ·024
VIII. ……	9·3–	+ ·009	——

It is impossible to represent in summary fashion the number of stars entering into these means, as the contributions of different observatories are not uniform. The photographic results cover the greater part of the stars in list I., but only a small part of list II.

The one point to which I would now call attention is that the photographic results do not support Prof. Tucker's conclusion that his first normal system is free from magnitude equation. On the contrary, they suggest that it possesses a magnitude equation of about 0^{s}·014 per magnitude. Nor do they bear out Prof. Tucker's conclusion that there is any considerable difference between the magnitude equations of his first and second systems. But they give a general support to the idea that there is relatively little error of this kind in the observations made with the Königsberg clock-driven micrometer, quoted by Prof. Tucker towards the end of his paper.

The results which I have given were obtained in the course of forming a standard system of comparison stars for the reduction of Eros observations, and cannot be published in detail for some time. But as the result of Prof. Tucker's interesting discussion of the visual meridian observations has been widely quoted, it may be well to give at once the main bearing of the photographic results upon his conclusions.

There is another indication that the observations for system I. are not likely to be free in the mean from magnitude equation. Prof. Turner has called attention in *Nature* to a magnitude equation between the systems of M. Lœwy and Prof. Tucker, which, according to the latter (Lick Bulletin 72), amounts to 0^{s}·004 per magnitude. Prof. Tucker speaks of this quantity as "very small," and in your June number it is included among "certain systematic differences which are seen not to exist." I think that it may be possible to hold the view that a difference of this magnitude is by no means small when it arises from weighting somewhat differently the same observations; and that if there is enough magnitude equation in some of the series to produce this difference when the weights are slightly varied, it is not likely that any system of weights would eliminate it altogether.

I have the honour to be,
Yours very faithfully,
ARTHUR R. HINKS.

Cambridge Observatory,
1905, June 26.

Hevelius and Cometary Orbits.

GENTLEMEN,—

In his letter in your last number (p. 254) Mr. Lynn says that Dörfel "completely confirmed the view expressed by Hevelius as a conjecture, that comets move in parabolas with the Sun in the focus."

Allow me to point out that this is doing Hevelius too much honour. He was solely guided by speculation, and believed the orbit of a comet to be nearly rectilinear, only slightly concave towards the Sun; and though he says that the resistance of the ether to the motion of the comet (which is shaped like a disc) bends the orbit into a parabola or a hyperbola, it does not occur to him that the Sun must be in the focus. Neither does he know that the velocity must be a maximum when the comet is nearest to the Sun, and thinks it may be greatest a little before or after the perihelion passage. He also seems to imagine that there are many kinds of parabolas more or less pointed at the vertex (see his 'Cometographia,' p. 684 at the top).

Hevelius was not a Pole, but a native of the free city of Danzig, which to a great extent had retained its independence though under the suzerainty of the King of Poland.

It may not be out of place to mention in your journal that the John Rylands Library at Manchester recently acquired a complete set of the works of Hevelius, including the excessively rare second volume of the 'Machina Cœlestis.'

Yours faithfully,
J. L. E. DREYER.

Armagh Observatory,
1905, June 15.

OBSERVATORIES.

ROYAL OBSERVATORY, GREENWICH.—Visitation Day, which happened on June 3, was made memorable by the regretted absence, through indisposition, of the Astronomer Royal, and for the first time within memory the Report was presented to the Board of Visitors by the Chief Assistant. As on the similar occasion last year, there was no Visitation Dinner, but the Established Staff met the Board of Visitors at lunch in the Octagon

Room. The general company numbered about one hundred and fifty. The following details are extracted from the Astronomer Royal's Report.

Transit-Circle.—The number of transits observed during the year was 15,842, of Circle observations 14,461. This is by far the largest number of observations made with this instrument in any previous twelve months. In the report for 1903, the similar figures were 13,270 and 11,470, which was the best record at that date. These observations are made chiefly of the reference stars for the Greenwich Section of the Astrographic Catalogue, and the Catalogue of reference stars will be completed this year. The Astronomer Royal specially remarks on the energy of the Transit-Circle observers.

The apparent correction for discordance between the observations of the nadir and stars observed by reflexion was found to be $-0''\cdot41$. The values found since 1892 range from $-0''\cdot20$ to $-0''\cdot41$. The correction for the R—D discordance was found to be $+0''\cdot047+0''\cdot267$ sin z.d., which is almost exactly the value found in the previous year. The correction to the tabular obliquity of the ecliptic as found from observations of the Sun in 1904 was $+0''\cdot11$. The apparent correction to the Right Ascensions of clock-stars found from the same observations was $-0^{s}\cdot046$.

The mean error of the Moon's tabular place (computed from Hansen's tables with Newcomb's correction) is $-0^{s}\cdot204$ in R.A. and $+0''\cdot01$ in N.P.D. The Report refers to Mr. Cowell's comparison between theory and the Greenwich meridian observations of the Moon, which has already been mentioned in these pages.

The Altazimuth.—This instrument is being used for the re-observation of the stars in Newcomb's Catalogue of Fundamental Stars and for observation of the Sun, Moon, and Planets, the instrument being used regularly for extra-meridian observations during the first and last quarters of the lunation. Since the beginning of 1905, observations of the crater Mösting A, as well as of the limbs of the Moon, have been observed with this instrument and also with the transit-circle. The total number of observations of the Moon is 104 with the transit-circle and 108 with the altazimuth—50 in the meridian and 58 out of the meridian.

During the year 949 double and 58 single observations have been made with the Reflex Zenith Tube, the number of different stars being 61.

With the equatorials, 42 disappearances and 15 reappearances of stars occulted by the Moon have been observed by one or more observers. The 28-inch Refractor has been used, as in previous years, for micrometric measurement of double stars, the total number of which measured during the year was 603; of these, 143 have their components less than $1''\cdot0$ apart and 60 less than 0″·5. A deterioration of the images of stars seen with the telescope was traced to the existence of a slight tilt between the lenses due to the dropping out of a small paper packing. With the 26-inch Reflector of the Thompson Equatorial, 65 photographs of Neptune were taken on 33 nights, and with the 30-inch Reflector 219 photographs of minor planets and comets.

The work on the Astrographic Catalogue, as far as the publication of rectangular co-ordinates of stars is concerned, is coming to an end. The plates are all measured, and these measures are printed as far as declination 77° N.—*i. e.* three quarters of the work of printing the measures is completed. It remains to print the remaining quarter and so complete the second volume, when there will remain the third volume, which will probably be a catalogue of the brighter stars in equatorial co-ordinates, to be done to finish the work. Reproductions of the Chart plates, enlarged to twice the original size—linear—are being made by a photographic process, and 197 reproductions of the plates in the three zones 65°, 66°, 67° have been distributed during the year.

The reduction of the photographs of the planet Eros to determine the Solar Parallax is proceeding, and it is expected that this work will be completed in about two months.

The solar activity has shown a great increase during the year ending 1901 May 10, and the Sun has not been free from spots on a single day during that period. The mean daily spotted area for 1904 was more than half as great again as that for 1903. It is proposed to complete the sun-spot ledgers which have been given in the annual volume since 1886, for the whole series from 1874 to date.

The mean temperature for the year 1904 was 40°·8, or 0°·3 above the average for the 50 years 1841–90. The rainfall for the year ending 1905 April 30 was 20·21 inches, being 4·33 inches less than the average. This small rainfall may be contrasted with the heavy rainfall of 35·42 inches for the corresponding period mentioned in the Report of 1904.

Harvard.—Prof. Pickering has to record the death of two of the most experienced computers of his staff—Miss Anna Winlock and Mrs. Eddy. The former was the eldest daughter of a former Director of the Observatory and had helped the late Prof. Rogers in his meridian observations with great skill. Mrs. Eddy has also done work in reducing meridian observations and was engaged on the zone 9° 50′ to 14° 10′ S. of the Astronomische Gesellschaft Catalogue at the time of her death. The photometric work, both for the detection and observation of variable stars, and for the formation of photometric catalogues, which has been a conspicuous part of the Harvard programme for many years, has been continued, and an extension is proposed. "The continuous work of twenty-five years, including more than a million and a half observations, has thus furnished a scale of magnitudes which has been extended systematically to fainter and fainter stars. It now appears desirable to carry this work still further, so that it shall include the faintest stars that can be seen."

The 11-inch Draper Telescope has been used to photograph spectra of stars. Twenty-one eclipses of Jupiter's satellites and five occultations have also been photographed with it. With reference to this instrument, Prof. Pickering writes:—"A new field of useful work has been found for it which will probably occupy its time for several years. Volume xxviii. part i. contains a careful study of the bright northern stars photographed with this instrument. It appears that a similar study may be made of the spectra of much fainter stars photographed with one prism and an exposure of about 60 minutes. This permits a much more detailed classification than is possible with the 8-inch Draper Telescope."

The discoveries made during the year from examination of the plates comprise nine variable stars, one star of the fifth type, one in which the Hβ line is bright, two gaseous nebulæ, three stars of the fourth type, and one with a peculiar spectrum, all by Mrs. Fleming; four variable stars by Miss Wells; and a star-spectrum, in which the Hβ line was bright, by Miss Cannon. "A study of the distribution of stars having different spectra has led to some curious results. It appears that the Milky Way is wholly due to stars of the first type, and that stars of the second and the third types are nearly uniformly distributed throughout the sky." Miss Leavitt discovered in the year ending 1904 September 30, to which the Report refers, 313 new variable stars from a study of the Bruce photographs, 152 having been found in the Large Magellanic Cloud. From a study of the chart-plates of long exposure Mr. Frost discovered a large number of asteroid trails and nebulæ, including 203 nebulæ in Virgo, where only 53 are given in Dreyer's Catalogue. The station of Arequipa, under the charge of Professor Bailey, continues to turn out a large quantity of photographic and photometric results.

Reference is made to the acquisition by the Observatory of the late Dr. Common's 5-ft. reflecting telescope. In 1902 a gift of 20,000 dollars was received with only one condition, viz. that the donor should not be made known. When the five-foot came into the market the greater part of the sum had been spent and the remainder was not sufficient to buy, but Mr. T. A. Common consented to dispose of the instrument upon terms that should lead him to be regarded as a contributor of a large part of the cost. The instrument is to be mounted in such a manner that the position of the eyepiece will be fixed and always directed to the South Pole, and the observer and recorder will work in a warm room. A photometer will be attached to this instrument, which is expected to be then the most effective existing instrument for measuring the light of all stars from the brightest to the faintest known.

STONYHURST.—Father Sidgreaves's publication for 1904 consists for the most part of the magnetic and meteorological observations of the year; the following is the gist of the astronomical section.

The prismatic camera employed for the spectra of stars has been removed from the Perry memorial telescope and mounted on the polar axis of the "Cross" 7-inch reflector (it will be remembered that the instruments of the late Col. Cross of Redscar were presented to the Stonyhurst Observatory), which is housed in a little observatory described as a strongly built wooden revolving shed. "Its chief excellence consists in its double shutter, which is also both doorway and window, and when fully open leaves the telescope objective in the free open air with a range of motion through nearly 180°, and at the same time sheltered from the wind. The shed is 9 feet high and rests on five broad flanged wheels running on a circular rail 10 feet in diameter laid on a concrete bed." With the instrument in its new gearing the first photographs were taken on June 26, and during the rest of the year 100 exposures were made. Many of these were experiments required by the new condition of things. But two series of spectrographs on β Aurigæ and γ Cassiopeiæ have been begun. The Perry memorial telescope is now more free in the early evenings for educational purposes, and on favourable nights both instruments are employed on the same stars for the simultaneous spectrographs mentioned in last year's report. The solar surface has been observed as usual, and spectrographs of the larger spots have been taken with the Rowland grating in the green and violet regions. Experiments have been carried on during the year for the photography of spot-spectra in the red region.

PUBLICATIONS.

'LES ECLIPSES DE SOLEIL.'*—This is a very attractive little book, containing a summary of the various observations that have been made during total eclipses, and hints to observers of the coming eclipse, dealing with the various problems that still await solution. A good deal of the matter is borrowed from Ranyard's great work on the subject, but the author has evidently made an exhaustive study of recent eclipse literature, and gives the leading results in a succinct and handy form.

He begins with some statistics of the sizes and distances of Sun and Moon; quoting Hansen's semidiameter of the Moon, $15' \, 34''\cdot09$, he says eclipses require this to be diminished by $1''\cdot35$. The accepted eclipse semidiameter is, however, considerably less than this, being only about $15' \, 31''\cdot6$.

He then quotes Du Séjour's figures for the maximum duration of totality, &c. It would have been better to have given Mr. C. T. Whitmell's results, which are based on more accurate values of the constants involved (Mon. Not. lx. no. 6). The maximum duration there found is $7^m\ 39^s$, 19^s less than that of Du Séjour.

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* Par M. G. Bigourdan. Paris: Gauthier-Villars. Price 3 f. 50 c.

The importance of timing the duration of totality is dwelt upon, this being an observation within reach of everyone. So far back as 1715 such observations were made, and they have, in Prof. Newcomb's hands, yielded valuable information as to the motion of the Moon's nodes.

The livid, unearthly hue of the landscape shortly before totality is explained by the absorption of the solar atmosphere, which arrests the blue and violet light from the Sun's limb, just as our own atmosphere does at sunset, but we must take exception to the author's suggestion (p. 42) that the corona and prominences play an important part in producing these colours, since the light given by them is immensely inferior to that of the thinnest crescent of sunlight.

The modern explanation of shadow-bands as due to wave-motion in our air produced by the rapidly changing temperature is explained, and the importance of noting the direction and velocity of the wind in this connection is indicated. Photography is suggested for studying them, but it is questionable whether any plates are rapid enough for an instantaneous exposure in such feeble light.

An unnecessary mystery seems to be made about Baily's Beads. M. Bigourdan says "they cannot be confounded with the irregularities of the Moon's limb." However, observations at Algiers in 1900 seemed to show clearly that the "Beads" are simply these lunar irregularities, which undergo an apparent enlargement through the widening of the solar crescent by diffraction and irradiation.

After some notes on the approach of the shadow and other attendant phenomena, a list of questions on the form of the corona is given, intended for the use of naked-eye observers, which should serve as a useful guide to them.

The next section deals with drawings of the corona or a part of it, with or without optical aid. This has been reduced to a system in recent eclipses, and the results show a marked improvement as compared with former years, partly no doubt owing to better knowledge of the types usually presented by the corona. M. Hansky's well-known diagram of coronal types is reproduced; the author attributes it to Mr. Maunder, who merely copied it into the B. A. A. Eclipse Volume for 1900; he himself called our attention to this *erratum*.

Coronal photography is next discussed, the following table of exposures being given for aperture $= \frac{f}{15}$:—

Prominences	$\frac{1}{60}$ sec. to $\frac{1}{45}$ sec.
Inner corona	$\frac{1}{10}$ sec. to $\frac{1}{5}$ sec.
Outer corona	$\frac{1}{2}$ sec. to $\frac{3}{4}$ sec.

It follows that with this aperture a stationary camera suffices except for large-scale pictures.

Passing to spectroscopy, the book contains an elementary description of the various forms of spectroscope and the purposes for which each is suitable. Among the problems noted as requiring further observations, are the distribution of the three constituents of the coronal spectrum (continuous, bright line, Fraunhofer) and spectroscopic study of the coronal rotation. Many observers have found a high rotational velocity, but the observation is a delicate one, and further observations are desirable.

Among minor phenomena, the "eclipse cyclone," the relative heating-power of the Sun's centre and limbs, variation of terrestrial magnetism during an eclipse are noted as subjects for investigation. In conclusion, we can strongly recommend the book to all who are about to view a total eclipse for the first time, while even experienced observers can hardly fail to derive a few new ideas and suggestions from its perusal.

NOTES.

A PROBABLE NEW STAR.—A recent Harvard College Circular (No. 99) announces the discovery of a "new" star from circumstantial evidence. That is to say, the star in question, RS Ophiuchi, is now old, and has hitherto been regarded as a variable, but Mrs. Fleming, from examination of its spectrum, pronounces it to be a star of the type of "Novæ." From the Draper memorial photographs it has been shown that the spectra of variables of long period are generally of the third type, Class M*d*, in which one or more of the hydrogen lines, $H\delta$, $H\gamma$, and $H\beta$, but not $H\epsilon$, are bright. The spectra of the bright novæ are very complex, but when faint only a few bright lines are visible. These consist of the hydrogen lines $H\epsilon$, $H\delta$, $H\gamma$, and $H\beta$, and one or more bright lines between 4600 and 4700. The spectrum of RS Ophiuchi is more nearly like the latter type. The star appears to have had the magnitude 10·9 before 1891, and to have varied slightly between that date and 1898, when it brightened up suddenly to magnitude 7·7 on June 30, and then diminished to 10·8 in October. It is now about the tenth magnitude and has slight variation.

THE WEATHER IN JUNE.—The outstanding feature of the weather during June 1905 was provided by the week extending from the 5th to the 11th inclusive, during which the rainfall amounted to 3 inches, and the sunshine to rather less than 4 hours, of which all but one minute occurred on a single day. The 6th was the coldest June 6 in 65 years, the maximum temperature being 6° below the previous lowest. The 7th was also the coldest June 7 in 65 years, and had also the least range of temperature for that day, while the range of 3°·1 on the 9th was, with one exception, the smallest known in June during the

65 years, the exception being a range of 2°·3 on 1903 June 19. The rest of the month was on the whole dry, warm, and sunny, until the last two days, which were decidedly cloudy and wet, the net result being a slight deficiency in sunshine, an average mean temperature, only one maximum over 80°, and an excess of rainfall which brings the total for the last nine months well over the average.

PHOTOGRAPHING THE CANALS OF MARS.—The following appeared in *Ast. Nach.* 4021, signed "Pickering":—"Lowell telegraphs: Canals of Mars have been photographed here by Lampland, Nilo Syrtis, Casius, Vexillum, Thoth, Cerberus, Helicon, Styx, Chaos, Liedus (Libneus ?), and others, some appearing on more than twenty negatives.

OBSERVATIONS OF EROS.—A telegram through the same channel in a later number of the *Ast. Nach.* (4027) states that Eros was photographed at the Arequipa station of the Harvard Observatory on April 11, 12, and 14, which appear to be the first observations of the planet since its recent conjunction with the Sun.

MR. MCCLEAN'S BEQUEST TO CAMBRIDGE.—It may be remembered that Mr. Frank McClean, by his Will, left the sum of £5000 to Cambridge University for improving the instrumental equipment of the Newall Observatory. The Observatory Syndicate have had the disposal of this under consideration, and of two alternative plans they have considered the better to be that the capital sum should be invested and that the annual income should be applied for the purpose of providing improved instrumental equipment for the Newall Observatory, or the capital fund may be drawn upon for the same purpose, subject, in all cases, to the approval of the Observatory Syndicate. The other plan suggested was that a large part of the capital sum should be at once appropriated to provide some considerable addition to the equipment, but this was not adopted.

THE SANTIAGO OBSERVATORY.—When the Astrographic Chart was begun about fifteen years ago, the Observatory at Santiago in Chili offered to take a hand, and a zone of 1260 plates in the zones $-17°$ to $-23°$ was allotted to it. But Santiago seems to have been unlucky. The then Director, Prof. Maturana, wishing to begin the work on the best possible plan, spent three years at the Paris Observatory in considering details with Admiral Mouchez, and then, having obtained all the knowledge of the work he wanted, he died. M. Obrecht, of the Paris Observatory, was appointed Director of Santiago in his stead, but political troubles occurred in the Republic and money was not forthcoming to carry on the work. It appears, however, that financial matters are now easy again, and the Santiago Observatory is about to begin its zone of the Astrographic Chart, and during the past month we have had the pleasure of making the acquaintance of the able Chief Asistant, M. Ernesto Greve, who is making a tour of European Observatories to make notes of what is being done this side of the Atlantic, and honoured Greenwich by visiting it first. M. Greve will take the opportunity of being in Europe to observe the Eclipse from Spain, in company with the Lick Observatory party.

CELESTIAL PHOTOGRAPHY FOR AMATEURS.—It so often happens that an amateur astronomer who is also a photographer asks how he can combine the two hobbies, that it seems not inadvisable to reprint the following paragraphs from the *Amateur Photographer*. The first is from an article by Mr. F. W. Longbottom, in the number for January :—

> To turn to the question of equipment, it may be said at once, that an ordinary fixed camera can only be used for snapshots of the sun or moon, or for time-exposures to catch meteor trails. For all other work, a telescope on an "Equatorial" stand is advisable. This need not be costly, as reflecting telescopes, so mounted, can be picked up, second-hand, reasonably enough, nowadays. Then a good old "Petzval" portrait lens should be obtained—these also are easily purchased. For all short exposure work on the sun or moon, the telescope should itself be used as a camera ; but for stars and nebulæ, the portrait lens, attached to a light, strong box camera, must be fastened on the top of the telescope tube, and the telescope then becomes simply a support and guide.
>
> Cross wires (or hairs) are placed in the focus of a "positive" eyepiece, and the telescope carrying the camera is pointed to a fairly bright star in the region it is desired to photograph. The image of this star must be put a little out of focus, and the expanded disc thus formed kept steadily on the cross wires, during the entire exposure. Even without any "slow motion," save what the hand can communicate, fairly long exposures can be made, but with a Hooke's joint-handle and tangent screw movement, ninety minutes would not unduly tax a patient man. Of course, a "driving-clock" is better still, though most of these require constant attention, and in any case the star must be watched, as corrections have to be made for varying refraction in the atmosphere.
>
> Speaking of plates, &c., is a delicate matter; but after a long series of experiments, Cadett's "Lightning" plates and their "Velox" developer seem to come out easily best for stars and nebulæ. For pictures of the sun and moon, where much subsequent enlargement is contemplated, any plate of medium speed and fine grain will do. All plates must be backed and kept as much from stray light as possible. In developing, the chief aim must be to attain detail and contrast.

The second is by Mr. Ernest Marriage in the number for April 25 :—

> The large size of the spot in the sun recently tempted me to make an attempt or two at recording it with an ordinary camera and a telephoto lens. A high-power negative lens revealed the fact that there was another spot of fair magnitude upon the sun's disc, besides the large one so readily visible to the eye through a smoked glass, and encouraged further investigation. The first exposure, made upon a process plate, showed that, even with a small aperture in the lens, there was no difficulty in obtaining a fully-exposed image. Bearing in mind the advantage of a plate with the finest possible grain for subsequent enlargement, I next tried a chloride lantern plate. The shortest exposure that could be made with the lens cap yielded upon development a strong negative.

Working details were as follows: Negative lens, 2·4 inches; 8-inch positive stopped down to F/32; distance from negative lens to plate, 17½ inches; magnification, about 8⅓ times; equivalent aperture, say, F/260. The diameter of the sun on the original negative is ·64 of an inch, and as every inch in the focal length of a lens means 1-100th of an inch in the diameter of the image of the sun, the equivalent focal length of the telephoto lens in this case was sixty-four inches. A weak and restrained pyro-soda developer was adopted, and the negative has a brown tint.

Oxford University has conferred the Honorary Degree of D.Sc. on Prof. G. H. Darwin, F.R.S., Plumian Professor of Astronomy in the University of Cambridge.

From an Oxford Note-Book.

The brief announcement of Civil List Pensions which appeared a few days ago contained one name which astronomers will be glad to see honoured, that of Mr. W. F. Denning, whose health has unfortunately been far from good recently. His severe astronomical work has told upon even his constitution, which was a strong one originally—so strong that he was perhaps tempted to take liberties with it on behalf of his beloved work. We shall all hope that the greater freedom from business anxieties which this well-deserved honour will ensure for him will materially aid in restoring his health. The pension was granted some months ago, but it was requested that no public announcement be made until the conclusion of the official year, although the news leaked out in a way which it was scarcely possible to prevent.

The same list honours the memory of two Professors of History lost by Oxford within recent years, Freeman and York Powell. The name of the former is widely known from his writings; but the reputation of the latter is almost confined to Oxford, for York Powell was one of those men whose influence within a limited circle of close acquaintances was extraordinary, but who failed almost entirely to communicate with those more distant. So careless in correspondence that the letter from high quarters offering him the Chair lay for days on his desk unopened, until it was followed by another; and so embarrassed in public that at his inaugural lecture, with enough material in his head, or even in the notes he held in his hand, for a dozen such lectures, he yet began to falter after twenty minutes, and dried up entirely at the half-hour; he was nevertheless so inspiring in friendly chat, and his fund of knowledge was so inexhaustible and so generously drawn upon for his friends and pupils, that at his death Oxford perceptibly shrank in size. His library was extensive. Too often we in Oxford have the sad experience of turning over the leaves of catalogues of such libraries, which must be dispersed on the death of those who gathered them so lovingly: our last office toward our dead friends is to glance through such leaves, or perhaps to run along the shelves of the books themselves, to see how we can best select material remembrances. York Powell had not many astronomical books, but I found among them Babbage's autobiography, 'The Life of a Philosopher.'

BABBAGE's name is the first in the list of Medallists of the Royal Astronomical Society, "for his invention of an engine for computing and printing mathematical tables" (1823 June 13). The machine was never completed, because he thought of a better one, and when he proposed to the Government to "scrap" the machine already started, which had cost some thousands of pounds, and start afresh, they simply gasped and closed their pockets. It seems to be characteristic of those who invent calculating-machines to think of something better before they can complete anything. Several years ago I inspected Babbage's abandoned machine in the South Kensington Museum in company with a gentleman who had sketched out a multiplying-machine which would be of the greatest value if we could get it made. But every attempt to make it resulted in the suggestion of new improvements, and the machine still exists only in the fertile brain of the inventor. "He was also embarrassed by the number of modes of producing the same action suggested by his prolific inventiveness," was written of Babbage (Mon. Not. xxxii. p. 104), and might doubtless be written of others who are capable of any good work in this field.

IN the 'Life of a Philosopher' there is a good deal about the Difference Engine. But there is also a good deal of anecdote about other matters. Here are two stories of Babbage's undergraduate days:—

> At another time we resolved ourselves into a Club which we called the Extractors. Its rules were as follows:—
>
> 1st. Every member shall communicate his address to the Secretary once in six months.
> 2nd. If this communication is delayed beyond twelve months, it shall be taken for granted that his relatives had shut him up as insane.
> 3rd. Every effort, legal and illegal, shall be made to get him out of the madhouse. Hence the name of the Club—the Extractors.
> 4th. Every candidate for admission as a member shall produce six certificates, Three that he is sane, and three others that he is insane.
>
> I mixed occasionally with a different set of whist-players at Jesus College. They played high; guinea points and five guineas on the rubber. I was always a most welcome visitor, not from my skill at the game, but because I never played more than shilling points and five shillings on the rubber. Consequently my partner had what they considered an advantage: namely, that of playing guinea points with one of our adversaries and pound points with the other.

LATER on in the book the following story occurs incidentally:—

> The Duke of Rutland and Theodore Hook having dined with the Lord Mayor were looking for their hats previously to their departure. The Duke, unable to find his own, said to his friend, "Hook, I have lost my castor." The Lord Chief Justice, Sir Frederick Pollock, was at that moment passing down the stairs. Hook, perceiving him, replied instantly, "Never mind, take Pollock's" (Pollux).

News from Mount Wilson will, I am sure, be generally welcome, and I gladly take advantage of a general permission to publish extracts from letters kindly accorded me by Prof. Hale. The following is dated May 29, 1905:—

"Everything goes on well at the Mountain, in spite of some stormy weather which has come much later than it should under normal conditions. From now on, however, we hardly expect to have any serious interruptions before autumn. The Smithsonian party, with Abbot in charge and Ingersoll as his assistant, is installing its apparatus for the measurement of the solar constant. Observations which have been made with a pyrheliometer indicate that the atmospheric conditions are likely to prove much more favorable than those in Washington. Observations are being carried on as usual at the Smithsonian Institution, so that if any real change occurs in the solar constant it should be detected simultaneously at the two stations.

"The Snow telescope continues to perform well, and the work with it is getting beyond the experimental period. I have no further news of the large prisms that are being made in Jena for the spectroheliographs, but the spectroheliograph we have fitted up for temporary use is giving good results. Barnard is getting beautiful photographs of the Southern Milky Way, and his tests of the definition at night, made while he is following with the 5-inch telescope, show that the seeing is very fine. He believes that Mt. Wilson will be an ideal site for the 5-foot reflector. Drawings for the mounting of the 5-foot are nearly completed, and the forging for the hollow polar axis, of nickel steel, is being made by the Bethlehem Steel Company. The Union Iron Works, of San Francisco, is to make all of the large castings and do the heavy machine work for the mounting. The smaller pieces will be made here in our new shop, where we are now installed. I spend two days of the week here, and the rest of the time on the Mountain. Our new telephone line gives such good service that I can dictate letters over it to my stenographer in Pasadena without fear of error."

There is a curious use of the word "horizontal" in the following extract from an account of the great naval battle of May 27–28. Is it perhaps a recurrence to the literal meaning?

> When the darkness set in, the Russian fleet was still edging towards the north; but the powerful Japanese fleet, in a horizontal line, lay across the enemy's bows, an effective barrier linked with guns.

An occasional correspondent kindly sends me the following extract from the *Westminster Gazette*, 1905, April 6:—

Foretold by Zadkiel.

One of the most remarkable forecasts given in 'Zadkiel's Almanac' of this year (writes an astrological enthusiast) is that which foretold the recent earthquake at Lahore. It is as follows:—"About the 74th degree of East longitude, where Saturn is on the fourth angle" (*i.e.* Lower Meridian), at the lunar eclipse of February 19, 1905, "a sharp shock of earthquake will soon be felt, most probably at the latter end of March and beginning of April." Lahore, it will be remembered, is situated in 74 deg. 16 min. of East longitude. 'Zadkiel's Almanac' for 1905 was published in October 1904, and this forecast can be read in it at page 68. Yet people still doubt astrology!

My correspondent continues:—

"Yet people still doubt astrology." This reminds one of Murphy's celebrated prediction of the coldest day in the winter of 1837-8 (a day in January), on which occasion his almanac sold immensely, regardless of the number of misses. People hardly understand that, if one were to write alternately through the days of a year simply the words Fine, Rain, what a number of times the "prediction" would be right.

PERHAPS others have seen the telegraphic message reported as sent by a native who was in the neighbourhood devastated by the earthquake, "Here Earth is quack how there?" which is worthy of being filed with the classical "Tiger on platform eating station-master wire instructions." I may perhaps subjoin another cutting from the same correspondent, giving quaint examples of similar imperfect English; from the *Daily News* of 1905 April 5:—

The Oriental point of view is always instructive. At Allahabad there has just been issued the report of the hospital for women, and the 'Pioneer' of that town prints a couple of grateful letters quoted in the report. The first addressed the lady at the head of the institution thus:—

"Dear She,—My wife has returned from your hospital cured. Provided males are allowed at your bungalow I would like to do you the honor of presenting myself there this afternoon, but I will not try to repay you—vengeance belongeth unto God.—Yours noticeably ——."

The second was in a strain of even more punctilious courtesy. It ran:—

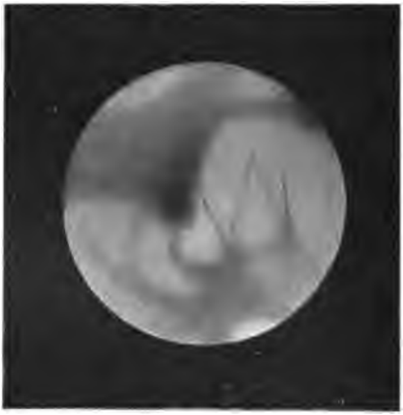
"Dear and Fair Madame,—I have much pleasure to inform you that my dearly unfortunate wife will be no longer under your kind treatment. She having left this world for the other on the night of 27th ulto. For your help in this matter I shall ever remain greatful.—Yours reverently ——."

MR. J. VINOGRADOFF writes from St. Petersburg as follows:—

I enclose herewith for review my work 'Theory of Cosmic Reason,' together with several advertisements for the trade, and would ask you to distribute the latter amongst those booksellers whom you consider most reliable and whose business turnover suggests the best chances of success.

The price of the book is half-a-crown, and I can thoroughly recommend it as worth the money. For instance, on the fifth page the author invents, perhaps unintentionally, the happy phrase "a scientific confiction." This will be convenient for describing those theories which have the whole-hearted support of their authors, but the mistrust of the public at large. The following extract from a chapter on "The Earth as an Animal Organism" will give a sufficient idea of the scope of the work:—

As every animal moves with its head foremost, and as the Earth moves before the Sun with its southern not its northern pole foremost, it is easy to conclude that the southern pole is that organ of the Earth which serves the purpose of a head.



MARS.

Drawn by W. H. Wesley from photographs taken at the Lowell Observatory, 1905 May 11.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXVIII. AUGUST, 1905. No. 360.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 June [illegible].

[illegible] A. C. D. Crommelin, B.A., [illegible]

Secretaries: J. A. Hardcastle, J. G. Petrie.

[illegible] Meeting were read and confirmed. [illegible] a [illegible] map presented [illegible] down to the [illegible] of the Society [illegible]

[illegible] Candidates for Membership [illegible] and [illegible] the election [illegible] declared.

[illegible] made by the Council [illegible] of the Council, to which [illegible] Meeting. The Scrutineers of the ballot were [illegible] and the President announced that it was proposed [illegible] Meeting about the middle of July if a [illegible] Members [illegible] desire to attend.

The [illegible] Messrs. G. [illegible] and [illegible] for the year.

[illegible] Company [illegible] by the [illegible] P. & O. [illegible]

277.

…graphs taken at the Lowell Observatory…
…May 11.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXVIII. AUGUST, 1905. No. 360.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 June 28.

President: A. C. D. Crommelin, B.A., in the Chair.

Secretaries: J. A. Hardcastle, J. G. Petrie.

The Minutes of the previous Meeting were read and confirmed.

The presents received included a beautiful map presented by the Hamburg Observatory, showing all the stars down to the 10th magnitude within about 8° of the Sun at the approaching total eclipse.

The names of ten Candidates for Membership were read and passed for suspension, and the election by the Council of seven new Members was unanimously confirmed.

The President read the nominations made by the Council for the ensuing election of Members of the Council, to which no additions were made by the Meeting. The Scrutineers of the ballot were then nominated, and the President announced that it was proposed to hold a special Eclipse Meeting about the middle of July if a sufficient number of Members intimated their desire to attend.

The Meeting then elected Messrs. G. Bruford and Gordon Miller Auditors for the year.

Mr. G. F. Chambers said that his efforts to arrange for a special steamer to the north coast of Spain had proved fruitless, but he understood that the Peninsular and Oriental Company were arranging that their ordinary steamer leaving Tilbury on August 25 should stop for a few hours on the central line, and would take passengers for the express purpose of seeing the eclipse, and would take them on to Marseilles, whence they could return to England by the next P. & O. steamer, sailing in about a couple of days.

Mr. D. W. Horner said as the great majority of the Members would not be able to go out of England to observe the eclipse, a few hints as to what they could do in the way of observations would be acceptable.

Mr. E. W. Maunder said there was another possible way of getting to the north of Spain which had recently been mentioned to him. He had been told that there was a regular line of steamers running from Bordeaux, calling at the northern Spanish ports, including the port of Gijón, on the line of totality. From England to Bordeaux the General Steam Navigation Company's service was, of course, available.

In reply to a question on the subject of the approaching eclipse, the *President* said he did not think the light of the Sun would be so veiled in England that Mercury would be seen. Venus should be visible, however, being not very much past its greatest brilliance. Mercury might be seen in a small telescope.

Mr. Maunder, at the request of the President, gave a *résumé* of two further papers on "Sun-spots and Magnetic Disturbances" recently communicated by him to the Royal Astronomical Society*.

The President, in thanking Mr. Maunder for this epitome of his papers, said he was sure the Members generally would join with him in congratulating both Mr. and Mrs. Maunder on the successful results of their long and patient analysis of both sun-spots and magnetic disturbances. It was also particularly satisfactory to have the difficulty raised by Mr. Adams some time ago cleared up, as to how it was they could both trace a diurnal effect in these magnetic disturbances, and also that they seemed to occur at the same local time all over the world.

Mr. Maunder read a paper by *Mr. W. Morton Sykes* on "The Zodiacal Light and the Gegenschein."

Mr. Keatley Moore asked what, roughly, in solar diameters, was the size of the Gegenschein.

Mr. Maunder said about 10° long (about 20 solar diameters) and about 5° broad, the greater extension being along the ecliptic.

The Rev. T. E. R. Phillips asked whether there was not a broad band connecting the zodiacal light with the Gegenschein, supposed to be a band completely round the ecliptic.

Mr. Maunder said that several observers had spoken of such a band, but he could not speak from personal experience.

The President said he was very glad that the observation of the zodiacal light and the Gegenschein had been taken up by the Members of the Association in Australia. They had a much better sky for it than we had in this country.

Dr. Downing gave a short account of a paper which he had read at the last Meeting of the Royal Society on the orbit of the Bielid Meteors *.

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* See the June number of this Magazine.

The President said the astronomical world was greatly indebted to Dr. Downing for having carried through this very tedious work. He was sorry the result was only a rather uncertain one as to whether they were likely to have any great display, but it was certainly better to be forewarned in good time.

Mr. Hardcastle gave Part 2 of his paper on "The Priming and Lagging of the Tides."

Mr. W. T. Lynn read a paper on "Orreries," which treated of the subject historically.

The President in the course of some remarks on Mr. Lynn's paper said that it had occurred to him that the cinematograph might be used to give a practically perfect representation of the motion of the planets in a manner that no machinery could do. If the accurate positions of the planets as calculated in the Nautical Almanac were plotted down on pieces of paper, so as to represent the solar system in plan at intervals of five or ten days, and these were photographed in a long strip, and then this strip was thrown on a screen by means of a cinematograph, all the planets would be seen moving at their proper speeds and distances, and the eccentricity of each orbit would be correctly shown.

Mr. W. Goodacre read a note on "The Green Flash," giving his observations of this phenomenon during a recent visit to India.

Mr. Hardcastle asked whether anyone present had ever seen the green flash at sunrise. It always seemed as if the Members of the Association looked for it at sunset, whereas he had never seen it at any other time than sunrise. The varying breadth of the amount of green was quite new to him, for it had always seemed the same when he had observed the phenomena, and he would describe the duration of the flash as being two seconds, but, of course, a far greater amount of the Sun was visible within two seconds of its rising than ought to be according to the movement of the Sun.

Mr. Goodacre said that he always saw the flash with unaided vision, though he had read that it was much better seen with a binocular.

Mr. Hardcastle said he would describe the tint as apple-green rather than emerald-green. He had often seen it through a field-glass.

Mr. Goodacre added that to him it seemed like a bright metallic green, very much like the green band in the ordinary solar spectrum.

After some remarks on the subject from the President, the Meeting adjourned till October 25th.

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* A *résumé* of this appeared in the July number of this Magazine, p. 279.

ROYAL METEOROLOGICAL SOCIETY.

An interesting feature in connection with the Agricultural Education and Forestry Exhibition at the Royal Agricultural Society's recent Show at Park Royal was the Section devoted to Meteorology. This Section was organized by the Royal Meteorological Society and attracted considerable attention. In the ground adjoining the Exhibition Building a typical climatological station was arranged, consisting of an enclosure 20 feet square with the instruments necessary for the equipment of such a station. An interesting exhibition of diagrams, maps, photographs, &c. was arranged inside the building, illustrating chiefly the effect of weather on agriculture. Among other things, the diagrams illustrated the monthly rainfall at Greenwich for 90 years, 1815–1904; monthly temperature of air and earth at 1 and 4 feet in various soils; temperature and rainfall when the crops of hay and of wheat were highest and lowest; distribution of rainfall according to altitude above sea-level; yield of wheat and autumn rainfall; temperature and barley crop; and maps showing annual distribution of rainfall and sunshine. The photographs illustrated clouds, lightning, damage by lightning, hail, damage by hail, snow, frost, floods, damage by gales, tornadoes, and whirlwinds. A number of instruments, such as barometers, thermometers, rain-gauges, sunshine-recorders, self-recording instruments, &c., were also shown.

Mr. W. Marriott gave a short address each day on "Meteorology in relation to Agriculture."

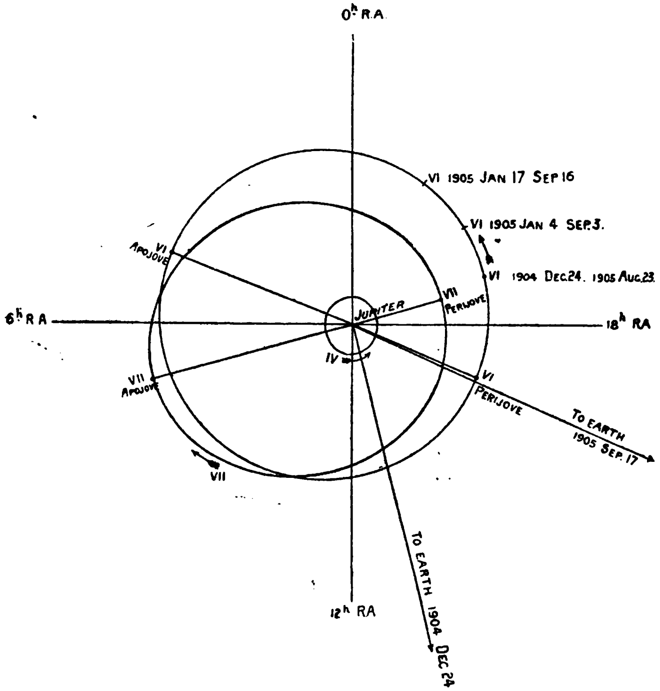
The Exhibition is part of a scheme which the Council of the Royal Meteorological Society are arranging for the extension of the knowledge of Meteorology among other Agricultural and Scientific Societies in various parts of the country.

The two New Jovian Satellites.

A few additional facts about these satellites are given in No. 102 of 'Publications of the Astron. Society of the Pacific,' which are reproduced below in the form of a table of elements. Two important elements, however (the longitudes of nodes and perijoves), are not given, so that the orbits can only be drawn approximately. The omission is difficult to understand, since these elements must have been determined with the others.

	Satellite VI.	Satellite VII.
Sidereal period	242^d	200^d
Mean distance in miles	6,968,000	6,136,000
Eccentricity	0·16	0·36
Perijove distance...........	5,853,000	3,927,000
Apojove „	8,083,000	8,345,000
Inclin. to Jupiter's equator ..	30°	30°
Direction of orbital motion ..	Direct.	Retrograde.
Computer	Dr. F. E. Ross.	Prof. Perrine.

It will of course be understood that the "Direction of orbital motion" cannot yet be considered as established, the period of observation being only 3½ months for Satellite VI., 2 months for Satellite VII. If these elements are correct we have the extremely interesting fact that the two satellites are moving in opposite directions in the same plane, and that their orbits intersect in two points. I have constructed the accompanying diagram, in which the size and shape of each orbit is correctly shown, but the positions of the perijoves have been inserted almost at random. That of Satellite VI. is probably not very much in error, but VII. may have its major axis in any direction. This uncertainty, however, does not affect the interesting fact that the two orbits intersect one another in two points, as will be seen on rotating the orbit of VII. about Jupiter.



Orbits of the three outermost Satellites of Jupiter.

The coincidence of orbit planes is probably not absolute, since in that case the satellites would probably have collided long ago; but they must occasionally make near approaches to one another, which will render the theory of their motions of great interest; the possibility of a collision even lends a species of excitement to the investigation.

Prof. Perrine notes that the solar perturbations of these satellites will be very large. The evection-coefficient of VII. will perhaps be as much as 4° or 5°, from its very large eccentricity, which is almost as great as that of Holmes' Comet (0·41).

The position of the North Pole of the orbit-plane appears to be about R.A. 9^h, N. Dec. 85°. From its close proximity to our North Pole it follows that the major axis of the apparent ellipse will always lie nearly east and west, being similar in this respect to the major axis of Saturn's ring.

I have constructed a rough search ephemeris of VI. during August and September, which should be near enough to enable photographs to be obtained. There is not enough material available to do this for VII., nor is it probable that any instruments in this country could reach it. It will be in inferior conjunction early in August in position-angle 4°, distance about 12′, motion eastward. VI. will be in inferior conjunction about the same time, but its motion will be towards the west.

Search ephemeris for Satellite VI.

Date.	Position-angle.	Distance.	
1905 July 19....	52°	19′	
29....	4	15	Inferior conjunction.
Aug. 8....	319	20	
18....	298	31	
28....	287	40	
Sept. 7....	280	48	
17....	274	54	
27....	268	56	West elongation.

The direct orbital motion of VI. does not lend support to Prof. W. H. Pickering's suggestion of initial retrograde rotation of the planets. The startling fact of the orbits of VI. and VII. being coplanar and yet their motion in opposite directions seems to set explanation at defiance; no plausible hypothesis has so far been suggested. The capture hypothesis seems to introduce at least as many difficulties as it removes, nor does it give any explanation of the orbits being coplanar.

It is strange that the last four satellites discovered have all some anomalous feature—Phœbe retrograde motion; ♃ VI. and VII. high inclination to primary's equator, VII. having also large eccentricity and retrograde motion; ♄ X. same mean distance as Hyperion.

The designations of the satellites of Jupiter reckoned outwards are V., I., II., III., IV., VII., VI. I hope that those concerned will remedy this most inconvenient arrangement by giving proper names to the three new discoveries and reviving the proper names Io, Europa, Ganymede, and Callisto for the four old satellites. This course was adopted for the Saturnian system when the numeration had got into a similar state of confusion.

A. C. D. Crommelin.

Real Paths of Meteors observed during the Lyrid Epoch, April 14–23, 1889–1903.

THE April meteoric epoch forms one of the most interesting periods of the year, though it rarely furnishes a display of striking brilliancy. There are a considerable number of minor showers visible at this particular season, but it certainly cannot be said that meteors generally are plentiful. As a matter of fact, the reverse is often the case, and the observer sometimes finds his patience sorely taxed, for in one of the beautifully star-lit skies of April he may not see a single shooting-star during an hour's watch! An autumn night will usually supply double the number of meteors seen on a spring evening. Not only the special shower of Lyrids, but the contemporary radiants of this well-known display are apt to be very feebly manifested in certain years. In others, however, the observer finds abundance of work, for there may be 25 or 30 meteors per hour when the Lyrids are active, and he must be expeditious if he would record the majority of those presented.

The frequent paucity of April shooting-stars has somewhat discouraged popular observation, but the habitual student of these phenomena finds much to attract him. The weather is often clear but variable in character, for it may bring the heat of summer, as in 1870 and 1893, or the frosts of winter, as in 1879. The minor showers of the period are very important. In late years there have been many slow, bright, long-pathed meteors from southern radiants at 189° −31°, 207° −10°, and 218° −29°. There have been occasional fireballs from Cassiopeia (20° +58°) and Perseus (48° +44°). Swift, bluish-white meteors have been directed from Hercules, while a few streak-leaving meteors have fallen from Cygnus. Ursa Major and Draco have also furnished many others of various types.

As to the main shower from Lyra, that has not provided us with a really abundant and brilliant exhibition for more than a century, for it was in 1803 that it last appeared under a striking aspect. The radiant-point, like that of the Perseids of August, displays a motion to the eastward, but the April shower has never been sufficiently investigated in regard to its centre of divergence. Its duration is brief, and the stream is of such tenuity that it is difficult to follow it satisfactorily on more than one or two nights just at the time of maximum.

But observations at this period have been pretty numerous in recent years, and it is thought that a list of meteors, the real paths of which have been computed by the writer*, may have some interest as helping to elucidate the radiants of the Lyrids and of the many other showers sprinkled over the heavens. The positions

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* The annual meteoric reports of the British Astronomical Association for 1900 and following years contain the results of similar calculations by Prof. A. S. Herschel at Slough.

No.	Year.	Date.	G.M.T.	Mag.	Height at appearance.	Height at disappearance.	Length of path.	Velocity per second.	Radiant-point. α.	Radiant-point. δ.	Notes.	Observers.
			h m		miles.	miles.	miles.	miles.	°	°		
1 ...	1895	April 14	11 44	1	87	71	107	49	316	+31	Early ζ Cygnid.	A. S. H., W. F. D.
2 ...	1893	15	9 52	☽	99	40	158	32	15	+59	Radiant inexact.	Many observers.
3 ...	1898	16	9 46	3–1½	72	55	52	...	212	−11	....	A. S. H., A. K.
4 ...	”	16	10 48	1¼–1	60	48	91	24	189	−31	R. = No. 28; S. of Corvus.	A. S. H., A. K.
5 ...	”	17	9 40	2¼–1	85	71	81	41	261	+14	α Ophiuchid.	A. S. H., A. K.
6 ...	”	17	9 55	4¼–3	57	55	24	Slow.	236	−13	η Librid.	A. S. H., A. K.
7 ...	”	17	10 28	5–3	72	70	34	23	176	−35	? R. = Nos. 4, 28.	A. S. H., W. F. D.
8 ...	”	17	11 12	3–2¼	67	44	24	Slow.	195	+40	α Canum Venaticid.	A. K., W. F. D.
9 ...	”	17	11 28	2	65	61	25	36	291	+13	Early ε Sagittid.	A. S. H., A. K., W. F. D.
10 ...	”	18	11 55	2–1½	65	58	19	32	274	+11	Early Ophiuchid.	A. S. H., W. F. D.
11 ...	1901	18	13 19	3–1	83	55	35	40	266	+33	A true early Lyrid.	A. S. H., W. F. D.
12 ...	”	18	14 47	2–1	58	44	23	15	247	± 0	λ Ophiuchid.	A. S. H., W. F. D.
13 ...	1903	19	10 6½	1–♃	59	26	65	18½	192	− 6	? R. = No. 40.	A. S. H., A. K.
14 ...	1901	19	9 28	♃–♀	56	52	40	25	226	−17	A brilliant Librid.	J. H. B. and others.
15 ...	1895	19	10 59	1–♀	91	43	97	33	269	+30	A bright Lyrid.	H. C., W. F. D., and others.
16 ...	”	19	11 46	1	77	70	40	50	300	+20	A bright Sagittid.	H. C., W. F. D.
17 ...	1889	20	10 16	2–1	51	46	22	29	301	+36	Early γ Cygnid.	A. S. H., T. W. B.
18 ...	1901	20	10 22	2–1	58	52	26	16	226	−17	α Librid. R. = No. 14.	C. L. B., J. C. W. Herschel.

19 ...	1900	20	11 44	1	70	63	53	35	210	−31	? R.=Nos. 24, 29, 38.	A. S. H., A. K., J. H. B.
20 ...	1893	20	11 56	3−1½	67	48	28	24	198	+ 5	*o* Virginid.	A. S. H., W. F. D.
21 ...	„	20	12 39½	2−1	75	68	35	49	314	+27	R.=No. 1; ζ Cygnid.	A. S. H., W. F. D.
22 ...	„	20	12 44	4−3	77	68	23	Swift.	292	+17	Early ε Sagittid.	H. C., W. F. D.
23 ...	1901	21	9 29	3×♂	62	58	53	30	311	+36	Early λ Cygnid.	W. F. D. and others.
24 ...	1900	21	9 54	2	53	52	15	30	218	−27	R.=Nos. 29, 38.	A. S. H., T. H. A.
25 ...	„	21	10 4	3−2	113	71	73	94	267	+46	Height abnormal.	A. S. H., T. H. A., J. H. B.
26 ...	1901	21	10 8	>1−>♃	79	51	68	40	268	+30	A bright Lyrid.	C. L. B., T. H. A., W. F. D.
27 ...	1889	21	10 16	2−1	72	54	42	28	218	− 5	μ Virginid.	A. S. H., W. F. D.
28 ...	1900	21	10 32	3½−2	60	54	50	17	188	−32	R.=No. 4; Corvus.	A. S. H., A. K., T. H. B.
29 ...	„	21	10 37	3	54	52	25	28	218	−31	R.=Nos. 24, 38.	A. S. H., J. H. B.
30 ...	„	21	11 22	1	66	44	55	14	21	+59	Well-observed Cassiopeid.	C. L. B., W. F. D.
31 ...	1893	21	13 38	5−3	62	45	37	Slow.	226	−11	? β Librid.	H. C., W. F. D.
32 ...	„	21	15 15½	3−2	80	65	33	V. swift.	299	± 0	Early η Aquilid.	H. C., W. F. D.
33 ...	1894	22	7 36	☽	80	17	120	30	48	+44	Brilliant Perseid.	Many observers.
34 ...	1903	22	10 23	1>1	45	26	20	6½	150	+49	Very slow motion.	C. L. B., A. K.
35 ...	1898	22	10 30	5−4	72	52	26	Slow.	252	+49	μ Draconid.	A. S. H., W. F. D.
36 ...	1903	22	10 36	>1−>♃	70	56	28	19	271	+34	A bright Lyrid.	A. S. H., T. H. A.
37 ...	„	22	12 32½	>1−♀	78	43	49	39	269	+32	A bright Lyrid.	A. S. H., A. K.
38 ...	„	22	12 35½	4−2½	49	44	20	20	216	−25	R.=Nos. 24, 29.	A. S. H., A. K.
39 ...	1897	23	11 21	3−1	61	48	22	22	213	− 3	? R.=No. 27.	A. S. H., W. F. D.
40 ...	„	23	11 37	3−1	62	53	15	21	196	± 0	? R.=No. 13.	A. S. H., W. F. D.

Observers:—T. H. A., T. H. Astbury, Wallingford, Berks.
J. H. B., J. H. Bridger, Farnborough.
T. W. B., T. W. Backhouse, Sunderland.
C. L. B., C. L. Brook, Meltham, Huddersfield.
H. C., H. Corder, Bridgwater.
W. F. D., W. F. Denning, Bristol.
A. S. H., A. S. Herschel, Slough.
A. K., A. King, Leicester.

in the table represent about 25 different systems, but a number of others are known (though doubly-observed meteors have not been gathered from them), and additional ones will be ascertained during the progress of future observation. We are far from having adequately investigated this inviting period, and it is desirable that special efforts be made in coming years to render our knowledge of its meteors more complete.

The following is an ephemeris of the Lyrid radiant, and it is hoped that its accuracy may be suitably tested. It is based on my observations of 703 meteors (186 Lyrids) during the years 1873-1904 April 15–25:—

	α.	δ.		α.	δ.
	°	°		°	°
April 15	263½	+33	April 21	271	+33
16	264¾	+33	22	272¼	+33
17	266	+33	23	273½	+33
18	267¼	+33	24	274¾	+33
19	268½	+33	25	276	+33
20	269¾	+33			

It is possible that the radiant is not visibly active on April 15, 16, and 25, as I have seen very slight evidence of it on the nights mentioned; but I have included them in the ephemeris, and based the positions on the motion of the radiant as determined on dates nearer the maximum.

Bishopston, Bristol,
1905, March 26.

W. F. DENNING.

Photographs of Mars.

BULLETIN No. 21 of the Lowell Observatory—"The canals of Mars photographed"—is of exceptional interest. It contains 6 photographs of the planet, each about ¼ inch in diameter, which show a decided advance in planetary photography. The photographs were taken on the 11th of last May with the 24-inch telescope, the aperture reduced to 12 inches or less, through a yellow screen and upon isochromatic plates. An arrangement for making a succession of exposures on the same plate at very short intervals is another improvement, for as the atmospheric tremors are constantly varying, it is quite possible that portions of the image which are indistinct on one picture may be clear on another. Mr. Lowell himself considers the reduction of aperture the most important matter, outweighing the evil of the longer exposure involved, since the effect of atmospheric disturbances is so much reduced. The photographs were taken by Mr. Lampland, who is to be congratulated on the success he has attained after a long series of attempts.

The photographs show the limb of the planet with tolerable distinctness, though it is somewhat deformed by the effects of irradiation, acting irregularly in accordance with differences in brightness of different portions of the planet. On examining the little images I am surprised to find how much detail they really show. That the main dark markings or "seas" should be photographed is nothing new, for these are given in a photograph (a copy of which is in the possession of the R. A. S.) taken at the Lick Observatory with the 36-inch telescope as far back as May 1890. The Lowell Observatory pictures, however, not only show the seas with much more defined outlines, but also several of the so-called "canals." Of these Mr. Lowell enumerates eight as being more or less well shown on his photographs, and I certainly think he is justified in his claim. I have examined and compared, with much care, the six images, of which enlarged prints are given in the 'Bulletin,' and I can plainly identify all the "canals" which Mr. Lowell mentions, as well as the various "regions."

The small photographs could not be satisfactorily reproduced from the prints by any photo-mechanical process, and I therefore give a drawing * which I have made from them. I have inserted in this *all* the details that I can make out with certainty on *any* of the six pictures. It is of course obvious that there should be a longer series for comparison, for while the stronger features are shown on all, some of the more interesting linear markings are only quite plainly seen on one (the sixth) of the photographs. It is these linear markings that are of the greatest interest, and if further photographs confirm what is shown on No. 6, there will be no reason to doubt their objective existence. On No. 5 I think I can make out some of them, though not clearly, but the lines shown on No. 6 closely resemble some given on what have seemed to me rather improbable drawings. In fact, these photographs go far to remove the scepticism I have always felt in regard to these features on the planet.

One remark of Mr. Lowell's I think is scarcely justified. He says:—"The negatives thoroughly confirm the eye in showing not only the existence of the canals but *the fact that they are continuous lines and not a synthesis of other markings.*" Does he not go too far in the words I have italicised? Doubtless a photograph has no imagination, but imperfect definition applies equally to photographic and visual observations. That which is seen or photographed imperfectly as a smooth continuous line *may* be full of small irregularities and may not be strictly continuous. It may, indeed, be a row of dots or discontinuous marks; we can only say certainly that *in the main* these run in straight lines. Major Molesworth, an assiduous and careful observer of Mars, says that at moments of best definition the "canals" appear to him more like streaks "made on very rough paper with a round-pointed crayon or stump, rather than an ink-line drawn with a pen" (Monthly Notices R. A. S. vol. lxv. p. 839).

It is to be hoped that Mr. Lowell will publish a further series of these interesting and valuable photographs. W. H. W.

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* See Frontispiece.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Lunar Eclipse of September 12, 1829.

GENTLEMEN,—

At a time when we are all rejoicing to know that the western limit of the total eclipse of next August, where it will take place earlier than at any other station, will be so well looked after, and are especially wishing that our friends Mr. and Mrs. Maunder will be favoured with fine and suitable weather at their proposed station in Labrador, it will perhaps not be without interest to refer to a previous American expedition to the same coast on the occasion of the famous eclipse of 1860, which took Sir George Airy to Spain in the 'Himalaya.' To observe that eclipse as far to the west as possible, a party (including Prof. Newcomb, the late Prof. Ferrel, and Mr. Scudder) travelled to the banks of the Saskatchewan, the outer edge of the belt of totality, but the weather was *not* propitious. To use Mr. Scudder's expression *, the outcome of the long journey was "to sit in a marsh and view the eclipse through the clouds."

The party which went to the coast of Labrador had to go about 5° further north than the projected Canadian party of this year—*i. e.* to latitude 60°, almost the same as that of Lerwick in the Shetlands. But there, too, the results were almost *nil* on account of clouds, and Lieut. Ashe, Director of the Quebec Observatory, alone succeeded in seeing any portion of the corona. The station selected was the small island called Aulezavik, opposite a harbour on the mainland, which has since been called in consequence Eclipse Harbour.

But another harbour has a prior right to this designation, and forms the immediate subject of this letter. It is about 10° still further north, or in latitude 70°, and is situated on the east coast of Boothia Felix, the northernmost part of the American continent.

Amongst the many expeditions sent out with the object of discovering a north-west passage from the Atlantic to the Pacific Ocean, two were made under the command of Captain (afterwards Sir John) Ross. The first of these was undertaken in 1818, and went up part of Lancaster Sound, but Ross came to the conclusion that it was impossible to proceed any further, and returned to England. The unfavourable comments made upon this, particularly after the return of Parry from the same locality in 1820, led Ross to desire to be sent again; but he did not acquire the means of doing so until 1829, when a small vessel called the 'Victory' was fitted up, partly at his own expense, but chiefly at that of Felix (afterwards Sir Felix) Booth. With this, accompanied by his nephew, afterwards Sir James Ross of Antarctic fame, he went out, but was stopped by the ice in Regent's Inlet, and spent the winter in Felix Harbour. In the following summer (that of 1830) they succeeded in reaching Victoria Harbour, further to the south, and wintered there. It was found impossible to extricate the ship from the ice; after a third winter she was abandoned, in May 1832. Ross and his men spent a fourth winter in a hut put together from the wreck of the 'Fury,' which had to be abandoned by Parry in 1825, at a place called in consequence Fury Beach, and in the summer of 1833 they were rescued by the 'Isabella'* (the whaler in which Ross had made his former voyage) and returned in her to England. It was in the course of a sledging expedition in this long sojourn that the commander's nephew, afterwards Sir James Ross, discovered the north magnetic pole on the 1st of June, 1831. But it was in the first year of this adventurous expedition, on the 12th of September, 1829, that the eclipse occurred which led Sir John Ross to give the name of Eclipse Harbour to that spacious harbour in Boothia Felix, which therefore has a right to the appellation prior to that in Labrador. "On that night," he states, "at midnight there was a visible eclipse of the Moon, but the weather did not permit of any observations." The spacious harbour was hence named Eclipse Harbour, and next day they passed Cape Allington (as they designated it) at its southern point.

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* Quoted in Mrs. Todd's 'Total Eclipses of the Sun,' revised edition (1900), p. 125.

A visible eclipse which could not be observed seems to us rather an odd description, and we are left in doubt whether Ross meant that it would have been visible had the sky been clear, or that there was too much cloud for useful observation. The eclipse was only partial, not quite half the Moon being obscured when greatest, which was at about 5 o'clock on the morning of September 13 by Greenwich time.

Yours faithfully,

Blackheath, 1905, June 8.

W. T. LYNN.

The Obscuration of "Bridges" in Sun-spots.

GENTLEMEN,—

Perhaps you will permit me to direct attention to a feature in connection with the large sun-spot now on the Sun's disc, which appears to me to be of interest. When I examined this spot on the 14th inst. the nucleus of the southern section of the spot was crossed by a narrow curved "bridge," which looked like a loop of brilliantly illuminated filaments resting on the dark background. On the 15th this "bridge" was still visible, but instead of being brilliant it was but faintly illuminated and appeared to be submerged in the dark matter of the nucleus.

So far there was nothing striking in the change. Such submerged, or partially submerged, more or less bright markings have been frequently seen, but, so far as my previous experience goes, such submergence has been followed by disappearance. I was therefore much surprised on again examining the spot on the 16th inst. to find this curved "bridge" restored to its original brilliancy and standing out apparently quite clear of its dark background. Moreover, as far as could be determined by memory, its shape had remained unchanged since the 14th inst.

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* The astonishment of her new commander may be imagined.

Now the interesting point in these observations appears to be this:—The "bridge" had a peculiar curved form, and consisting as it did of a very slender filament it appears difficult to believe that it could have retained its form during obscuration had that obscuration been caused by any other than gaseous cloud-like matter. Indirectly therefore the facts now recorded may afford some indication of the character of the material forming the dark nuclei of solar spots.

Unfortunately, I have not been able to examine the spot since the 16th inst., but some of your other readers may have done so, or the photographs taken at Greenwich may enable the history of this particular "bridge" to be traced further. A comparison of photographs taken on the 14th and 16 inst. would be particularly interesting, as it would show if the filament had really between those dates retained its shape as completely as—speaking from memory—it appeared to me to have done.

I may add the observations above referred to were made at Outwood, Surrey, with an 8-inch refractor and using a Dawes' solar eyepiece.

I am,
Yours sincerely,

Kensington, 1905, July 18. WILLIAM H. MAW.

[On the Greenwich photographs of July 14 a "bridge" is seen as a fine curved line, such as Mr. Maw describes, crossing this section of the nucleus. It is not seen on the photographs taken on the next day. On July 16 a very faint marking is seen near the place, but it is difficult to say if this is the same as was seen two days earlier.—EDS.]

Continued Visibility of the Helium D_3 *Absorption Line on the Disc of the Sun.*

GENTLEMEN,—

You were kind enough to insert in the June number of your valuable Journal a letter of mine containing some remarks as to the repeated visibility of the Helium D_3 absorption line on the disc of the Sun, and with your kind permission I should like to add a few further instances of its observation made since. When I addressed you nearly two months ago, I had myself no idea that the visibility of this particular line would be so frequent as it has proved to be.

I have seen it well since May 13 on no less than 12 days, of which there were two observations of several days in succession in the same disturbance. On one day it was plainly visible in two different disturbances simultaneously, and on several more days its presence was urgently suspected, but passing cloud and other circumstances prevented certainty. If we, then, add to the above observations those already reported, we must see that the phenomenon, although possibly depending to some extent on special circumstances, is by far more frequent than has hitherto been believed. It is really remarkable to reflect how an utterance once made as to its scarcity, should prevent many from ascertaining for themselves its truth. I cannot get myself to think that the recent frequence of the appearance of the D_3 absorption line on the disc of the Sun is in any measure due to a general change of the solar conditions since Young recorded the first observation in 1870. Then there is a record by Hale and one by myself in 1899 (see Journal of the B. A. A., vol. ix. No. 6), and now I have them in shoals. At the same time I admit the trying circumstances with which this observation is connected and which may go far to explain the comparative inattention which this phenomenon has received in past years.

The line is placed in the spectral band where the brightness is practically at its maximum, and its neighbourhood must be examined with an instrument of considerable dispersion. This difficulty is not lessened by the circumstance that the line is difficult to localize, but there happens to be an atmospheric vapour absorption line near by on the red side, and while this line helps as a guide, one must at the same time beware of deceptions arising from its very presence. Still the appearance of D_3, if present, is so different to this atmospheric line, that a practised eye is not so easily taken in. Its length is of course determined by the particular circumstances of the disturbance under observation and it may appear in detached bits, whereas the telluric line referred to crosses over the entire width of the gammut. Again, D_3 shows widening effects, and the atmospheric companion does not; it shows even distortion and part reversals into brightness, which, again, the atmospheric line does not. When D_3 is present, it intrudes upon the eye with a curious effect, like that of splitting a pea-pod, close to the violet side of the atmospheric line referred to, and which I believe to be Rowland's λ 5876·338, whereas D_3 is given at λ 5875·98. The spectrum is suddenly broken into by dark D_3 at the position it occupies, and no such action would accompany the atmospheric line, which is always there, and the steady serenity of which is never disturbed by widening, displacements, and knot-effects, which sometimes extend over very considerable areas. Young's observation of dark D_3 seems to have been over a spot, but in no case I have seen it, so far, was this the case, the line appearing invariably in the agitated area just behind a spot or in the intervening space of an active spot-pair, and Prof. A. Fowler confirms me in this characteristic of the phenomenon.

A small spot came round the eastern edge of the south limb on July 1–2, being preceded on the limb by a splendid array of active prominences, and in this spot, or rather immediately behind it, D_3 was well seen. Contrary to expectations, very great developments of activity of this spot and its immediate follower, which consisted of a close pair of tiny black spotlets, did not take place. Its activity soon subsided, and although I have seen to-day in the vicinity of the leader and of the following small pair signs of renewal of activity by considerable displacements of the hydrogen absorption lines, I have not seen D_3 again in this disturbance, but had nevertheless strong reasons to suspect its presence. A few days after the spot referred to appeared in S.L., another disturbance arrived in the same latitude, and on the morning of July 6, when the new arrival was just well within the disc, D_3 showed strongly in its immediate rear, and in due course a second spot appeared, making up a pair, which has since tremendously developed and shown activity, quite as intense as that of the recent naked-eye group and that of February last. In this group D_3 has been on every day since its appearance, *i.e.* I believe it has been there constantly, with intermission of intensity only, synchronous with the special outbursts which this fine disturbance has shown on all days of observation up to now. This time I happened to see D_3 first, and from that concluded the likely appearance of the hydrogen lines; and truly on inspection they did present an extremely serrated appearance, while the sodium and magnesium lines were also affected to an intense degree — displacements amounting to 5 and more tenth-metres constantly occurring towards both, the more refrangible and the less refrangible end of the spectrum, the lines over the agitated area looking like a saw. Yesterday at 2.45 P.M. a specially brilliant impulse occurred, and immediately abutting on the south-eastern edge of the leader spot the hydrogen lines shone out for just about 3–4 minutes like a brilliant star with extended wings towards the red *and* the violet end, indicating great pressure. All the lines affected became extremely serrated, the displacements being in all directions, bright and dark, and D_3 had an enhanced distinctness, being seen dark as well as bright. I repeat here, what Prof. Fowler also has found, that the appearance of the hydrogen lines is a good indicator as to the likely presence of dark D_3. Jewell says: "D_3 does not occur as a dark line in the solar spectrum, but is sometimes, if not always, present as a very weak *bright* line. This is shown by a study of the best photographs of this region of the solar spectrum." I truly regret that the photographic camera has so far not been brought into action for catching D_3 dark, as the elucidation of any "mystery" still attaching to this line may thereby be brought about. I feel sure, after what I have seen so far, that D_3 must also show absorption in areas on the disc without a spot being necessarily there. I have seen many a time the same serrated appearance of the hydrogen lines in other parts of the disc, and, judging from the clue this affords for the likely seeing of dark D_3, this line may possibly be seen almost anywhere, at any rate within the activity zones.

It seems to me that, in the case of helium, we are dealing with a layer of gas, not so much of small emissive power (at the edge of the Sun it truly shines brilliant enough), but one which has the same temperature throughout its depth, which many believe to be very shallow. In such a case no absorption line could be expected under normal circumstances, but over a disturbed area these normal circumstances are upset. The hydrogen lines unmistakably indicate that great masses of the chromosphere are flung upwards, cool down in the higher regions, and descend again at equally prodigious rates of speed. Similarly exceptional quantities of helium are transported up and down and, where they were cooled, show an absorptive effect on the helium below, quite analogous to the hydrogen. There is nothing forced in this assumption, considering the great affinity and constant companionship of these two elements.

I venture to submit that, in face of these many observations of D_3, some of the mystification with which the behaviour of this element in the Sun and stars has been endowed should cease. To me the phenomena connected with helium as against other gases in the Sun (such as, for instance, hydrogen) seem to me to show a differentiation "in degree" rather than of "kind," and therefore there remains an intrinsic difficulty in catching sight of D_3, to which the circumstances enumerated at the head of this letter contribute also. But this should be no deterrent to observers, better situated and equipped than what I am, from vigorously prosecuting inquiry into this subject, which has, as already stated, also its bearing on the helium stars.

Just as I finish this letter, I walk over to the instrument on the lawn and have another good glance at dark D_3 (for about the hundredth time within this year alone), and I reflect with a pang of regret on all the lost opportunities, all because someone wrote it could hardly ever be seen. Possibly Prof. Hale will some day delight us with photographs of helium-flocculi and helium absorption patches, and it is my conviction that he will never expose his plate uselessly to any agitated spot group if he can steer the second slit clear of any other line close to D_3.

Finally, I wish to tender my apologies to the Editors for the great length of this letter; but I trust they will deem the subject under discussion of sufficient importance generally, and equally so to intending observers. I remain, Gentlemen,

Yours respectfully,
ALBERT ALFRED BUSS.

2 Lansdowne Terrace,
Grosvenor Square,
Ashton-on-Mersey.
1905, July 9.

Captain Edmond Halley's Letters (1698–1700).

GENTLEMEN,—

There are said to exist *thirty-four* letters written by Dr. Edmond Halley during his two voyages made while he was a Captain in the Royal Navy. These communications, which are described as "interesting" by Miss A. M. Clerke in her sketch of Halley (Dict. Nat. Biog. xxiv.), are addressed to the Secretary to the Admiralty, and are preserved, in the original, in the Public

Record Office, Admiralty Section, *in two volumes*, under heading "Captains' Letters, 1698–1700." That portion of the foregoing data which is italicized was procured through private sources. There is reason to believe that the letters might be published by the Smithsonian Institution or by the Department of International Research in Terrestrial Magnetism established by the Carnegie Institution of Washington. Will anyone reading these lines, who has examined the documents, oblige the undersigned with an expression of opinion as to their scope and probable present value to science or its history? While they may be somewhat discursive in character, the writer anticipates their inclusion of scientifically valuable comments.

Very truly yours,

EUGENE FAIRFIELD MCPIKE.

Member of the Bibliographical Society of America.

1 Park Row, Chicago, U.S.
1905, July 5.

Peters' Catalogue.

GENTLEMEN,—

From expressions which have reached me I infer that my letter on the subject of Mr. Knobel's connection with the Peters' manuscript, which was printed in the *Observatory* for April, did not make clear my attitude in the matter. I would like to set the matter at rest by a few direct and specific statements.

1. The idea that, in my 'Reminiscences,' I meant to insinuate that Mr. Knobel had no claim to Peters' manuscript, is entirely ungrounded. If I wrote anything legitimately admitting of such an interpretation (which I am not ready to admit), I am very sorry.

2. I am so far from wanting to make out an adverse case that I do not wish to bring forward any evidence tending to abridge or belittle any claim that Mr. Knobel may have on the work.

3. I have reason to believe that the executors of Peters' estate acceded to Mr. Knobel's request for the manuscript for the reason that, quite apart from any claim that he might have on it, he seemed to be the only person able and willing to bring the work to completion. I quite concur with the soundness of this reason for their action.

4. I also learn that there is no authority in this country disposed to claim the manuscript or the right of publishing it. The whole matter is, therefore, in Mr. Knobel's hands.

5. I think I voice the unanimous desire of all concerned in expressing a wish that the work be brought to completion and published as soon as practicable. Very respectfully,

SIMON NEWCOMB.

1620 P Street N.W.,
Washington, D.C.,
1905, June 12.

Names for Satellites.

Gentlemen,—

Whilst the members of the ever-increasing crowd of Minor Planets are promptly supplied with names, we have, as yet, no name for the satellite of Neptune, discovered nearly sixty years ago; and for the satellite of Jupiter, discovered in 1892, we have only a number. Now that fresh members have been added to the families of Jupiter and Saturn, the lack of distinctive names, such as those so happily given to the satellites of Mars, is a decided inconvenience.

Should not the discoverers of these recently found Moons be asked to name them? Their right to do so will not be disputed. In the case of Neptune's moon, unfortunately we cannot appeal to Mr. Lassell; but if this satellite may not bear his name, would it not be well for our leading astronomers (at their next conference) to find some suitable title? Yours faithfully,

Leeds, 1905, July 6. Chas. T. Whitmell.

Recent Planetary Observations.

Gentlemen,—

There were comparatively few really favourable opportunities for the critical examination of Mars in May and June. The sky when clear in the daytime often clouded over at night, and on those occasions when weather permitted observation definition was generally bad.

On June 24, July 6, 7, and 8, however, good views were obtained, and a mass of detail, too imperfectly glimpsed and too complicated in character to be accurately depicted, was seen. On July 8, 8^h 30^m to 9^h, the image was very steady, and the markings strikingly displayed in my 12½-inch Calver reflector with a power of about 300. I should have liked all those who are sceptical as to the existence of the features termed "canals" to have looked into my telescope on this occasion. The "canals" were observed just as certainly and steadily as the fainter belts of Jupiter. Among others I identified:—

Typhonius.
Protonilus.
Phison.
Orontes.
Euphrates.

Cantabras.
Deuteronilus.
Gehon.
Hiddekel.
Eulæus.

I regard "canals" as a name incorrectly expressive of the irregular streams of shading, knotted pretty abundantly, and by no means straight or narrow, which run over the northern hemisphere of Mars. To my eye they are bands varying in tone and breadth everywhere, and induce the impression that they are perfectly natural features. The idea that they are clearly cut lines, suggestive of artificial origin, may be dismissed as a mere conjecture unsupported by reliable evidence.

Some of the features on Mars undergo temporary changes of a rather conspicuous character. As compared with the aspect in 1903, the markings have, I believe, been somewhat fainter in recent months. But the altitude of the planet has been 17° less than in 1903, and the greater diameter ($17''\cdot4$ in 1905 as against $14''\cdot6$ in 1903) has not offered a compensating advantage. The increased atmospheric interference and poorer definition which have recently affected the study of Mars may well have caused the markings to appear more diffused and faint than in 1903, but some of the changes observed have been probably real though far from permanent. Thus Acidalium Mare has been much fainter of late than the deep tone it presented in 1903. The North Polar cap has been scarcely recognizable this year, whereas it was vividly luminous at the last opposition.

The 12½-inch reflector has afforded me some magnificent views of Saturn in the morning twilight. The brightest part of the ball is not now the equatorial zone, but the region north of the broad or multiple belt in the northern hemisphere. No conspicuous markings were observed on June 23, 24, 27, July 6, or later dates, but a small white spot was detected on the N. edge of the great belt on July 6, and estimated central at $13^{h}\ 40^{m}$. It was followed by a darkening of the border of the belt, and in this respect resembled the aspect of Jupiter, in which we usually find white and dark spots in close contiguity on the edges of the dark belts.

On July 13, 16, and 21 I saw a small white N. Temp. spot on Saturn as follows:—

	Transit-Time. h m	Longitude System II. °
July 13	13 20	72·7
16	15 35	67·3
21	12 35	69·5

On June 24 I estimated the Red Spot Hollow on Jupiter as central at $15^{h}\ 43^{m}$, and on July 6 at $15^{h}\ 40^{m}$, so the longitude was $25^{\circ}\cdot1$ and $24^{\circ}\cdot8$ respectively. As I found the longitude on March 22 $25^{\circ}\cdot3$ and on March 27 $24^{\circ}\cdot6$ (*Observatory*, April 1905, p. 188), the motion during the last three months, while Jupiter has been too near the Sun to be visible, has conformed precisely with System II. of the ephemerides based on a rotation-period of $9^{h}\ 55^{m}\ 40^{s}\cdot63$ ('Monthly Notices,' 1905 January, pp. 304–315).

My observations of Jupiter on June 24 and July 6 were made with a 10-inch reflector, powers 205 and 300. The Rev. T. E. R. Phillips, of Croydon, informs me that with his 9¼-inch reflector he saw the Red Spot Hollow central on July 6 at $15^{h}\ 38^{m}\cdot5$, longitude $23^{\circ}\cdot9$. This agrees very well with the observation I obtained on same date.

Yours faithfully,
W. F. Denning.

Bristol, 1905, July 22.

OBSERVATORIES.

Cambridge.—Judging by its place in Sir Robert Ball's Report for the year ending April 30 last, the reduction of the photographs of Eros by Mr. Hinks is now the chief work at the University Observatory, and it is stated that provision has been made from the Government Grant Fund of the Royal Society for assistance in computation of a system of standard stars necessary for this reduction. The meridian circle, which is now in the charge of Mr. W. E. Hartley, M.A., has been altered, repaired, and adjusted, and since 1904 September 16 1145 observations of stars were made on 57 nights. The Sheepshanks Equatorial has been used by Mr. Hinks and Mr. H. N. Russell, of King's College, a Research Student of the Carnegie Institution, who has been temporarily an honorary member of the staff, to take photographs for the determination of stellar parallax. Mr. Russell has measured 35 plates during the year. A paper on this subject, it will be remembered, was read by him at the June Meeting of the Royal Astronomical Society.

Mr. Bryan Cookson, Mackinnon Student of the Royal Society, has returned to Cambridge from the Cape of Good Hope, and has set up his floating photographic zenith-telescope in a new building designed to avoid the effects of temperature-changes and of wind. A complete observing programme has been drawn up for the determination of the Constant of Aberration and Variation of Latitude by Küstner's method, but the weather was very unfavourable for this work since February, when the observations were begun, and it seems that the programme will take longer than expected.

The University has established a "Special" Examination in Geography, embracing Field Astronomy and Surveying. Instruction is given in these subjects by Mr. Hinks, and the Observatory is able to give facilities for the astronomical work, of which advantage has been taken by six students. On fine Saturday nights during the Michaelmas and Lent Terms the Northumberland Telescope, and sometimes the Sheepshanks Telescope, have been placed at the disposal of members of the University and their friends, and considerable numbers have availed themselves of this privilege.

Mr. Newall's report, as might be expected, refers wholly to spectroscopy, and it is stated that 131 photographs of stellar spectra have been taken during the year. Reference is made to a paper communicated to the Royal Astronomical Society on "Velocity Reference Stars" observed in 1903 in cooperation with other observatories, and to another on the design of spectrographs from the point of view of tremor-disks.

In the latter paper it is shown that in consequence of atmospheric tremor a wide slit must be used in the spectrograph, if efficient use is to be made of the light collected in the star-image formed on the slit; and the larger the aperture of the equatorial is, the wider the slit must be. But the wider the slit is, the larger must be the train of prisms to secure the required perfection in the photographed spectra; and consequently the greater is the loss of light by absorption in the glass prisms. Hence we have conflict between two elements in the design of spectrographic installations; and it is shown, in the paper referred to, that in the use of equatorials of large aperture, it is better to sacrifice light by making the slit of the spectrograph narrow, and so to use shorter prism trains in order to avoid excessive absorption. By numerical calculations it is shown that when the aperture of the equatorial is 25 inches the turning point has been reached at which a further increase in aperture is attended with increasingly disappointing gain in the brightness of the photographic spectra. It is further shown that the use of a properly chosen diffraction-grating may enable us to photograph fainter stars than are accessible to prismatic spectrographs.

Mr. Newall has made preparations for observing the eclipse of the Sun in Algeria.

Bidston, Liverpool.—During 1904 the transit instrument was used continuously for the determination of time, and 2586 stars were observed during the year on 142 nights. Comets Brooks, Giacobini, Borelly, and Encke were observed with the equatorial so long as they were visible. A photographic plate of the Cluster in Hercules, taken at the Yerkes Observatory, and the positions of over 2000 stars have been determined. This is a brief summary of the astronomy in Mr. W. E. Plummer's Report, the remaining and larger part is devoted to meteorological and seismological observations. It is said that the number of earthquakes recorded here is somewhat greater than at some other British stations.

PUBLICATIONS.

Astronomers of To-Day *.—Biographies are generally interesting reading, and when astronomers form the subject they always seem to be instructive, for the biographer necessarily notes the details of the man's work, and the reader learns many facts—often a complete history—of the special department in which the astronomer has made a name. Such is the case with the book now before us, in which Mr. Macpherson has collected sketches of the lives of thirty-two astronomers—it might have been written "living" astronomers, if it had not been for the recent loss, Otto Struve—which, with two exceptions, appeared originally in an Edinburgh newspaper. The author explains in his preface that the astronomers who are thus distinguished supplied the biographical details, read and corrected the articles, and supplied the portraits, and that he has drawn largely on Miss Clerke's books

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* 'Astronomers of To-Day and their Work.' By Hector Macpherson, Junior. With Twenty-seven Portraits. Gall & Inglis: London and Edinburgh, 1905. Price 7*s*. 6*d*. net.

and on the 'Monthly Notices' for his information, so that assurance is given of the correctness of the facts, and the reader will not be long in recognizing the skill and care with which they have been selected and compiled. There is a useful subject-index at the end, and twenty-seven of the sketches are illustrated by portraits. Of the thirty-two astronomers, nine are American, ten are British, four German, three Italian. There are two sketches of Frenchmen and two of Russians, whilst Sweden and Holland have each one representative. To show how the selection has been made, it may be of interest to give the British list in the order they appear in the book:—Huggins, Lockyer, Copeland, Ball, Miss Clerke, Gill, Gore, Darwin, Denning, Maunder.

NOTES.

Comet Notes.—It appears certain that the orbit of Comet 1905 *a* is elliptical, with a period of about two centuries. Mr. Banachiewicz, of Warsaw, gives the following elements in *Ast. Nach.* 4027; they are derived from observations on nine days extending from March 27 to April 28:—

T	1905 April 4·0810 Berlin M.T.	
ω	$358^\circ\ 12'\ 17''$	1905·0.
☊	157 27 42	
i	40 11 21	
log q	0·047017	
log e	9·985644	
Period	200·6 years.	

Herr Wedemayer has deduced similar elements with a period of 279 years, that of the Crawford-Maddrill orbit being 231 years. A weighted mean is 222 years, which we may take as the most probable value.

The comet was observed by Prof. Aitken on May 21, the errors of the Crawford-Maddrill ephemeris being

$$\Delta\alpha \cos\delta = +0^{\rm s}{\cdot}32, \quad \Delta\delta = +3''{\cdot}5;$$

these small residuals render the ellipticity of the orbit practically certain. A. C. D. C.

Minor Planet Notes.—433 Eros was photographed at Arequipa on April 11, 12, 14 last. Its position on April 11 was R.A. $26^{\rm h}\ 36^{\rm m}{\cdot}6$, S. $25^\circ\ 4'$, on April 14 R.A. $20^{\rm h}\ 42^{\rm m}{\cdot}2$, S. $24^\circ\ 39'$.

It was also observed visually by Prof. Millosevich $1\frac{3}{4}^\circ$ E. of δ Capricorni, on June 12 at $14^{\rm h}\ 32^{\rm m}\ 24^{\rm s}$ Rome M.T., its position being R.A. $21^{\rm h}\ 48^{\rm m}\ 41^{\rm s}{\cdot}74$, S. Dec. $16^\circ\ 41'\ 35{\cdot}3$, the seconds of calculated R.A. and Dec. being $41^{\rm s}{\cdot}78$, $34''{\cdot}2$, a most satisfactory agreement. In this calculation the value of ω has been taken as $177° 44' 21''.9$ instead of $177° 24' 21''.9$, as printed in the 'Berliner Jahrbuch' for 1907.

The following planets have received names:—485 Genua, 486 Cremona, 494 Virtus. It is worthy of note that as 487 is Venetia, three successive planets are named after Italian towns; besides these there are 472 Roma and 512 Taurinensis (Turin). Dr. Berberich has discovered that a planet photographed on 1902 February 24, and at first supposed to be 462 Eryphile, was really 537, discovered last year.

The following new planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Magnitude.
QO	1905 May 28	Götz.	11·8
QP	„ 28	„	12·5
QQ	June 28	Wolf.	12·7

The long-lost planet 156 Xanthippe is at present under observation, being of magnitude 10·4. A. C. D. C.

OBITUARY.—We regret to hear of the death, at a comparatively early age, of Mr. W. E. BESLEY, a keen meteoric astronomer. In the papers published by Mr. Denning in this Magazine from time to time, Mr. Besley's name frequently figured as that of an observer, and in April 1899 an excellent article by him on the Lyrid Meteor-shower appeared in these pages. In 1900 he succeeded Mr. Denning as Director of the Meteoric Section of the B. A. A., but had to resign that position soon after on account of ill-health. He also did valuable work for the Variable-Star Section of the Association. Mr. Besley, who came from Exeter, held a post in a London Government office. He was elected a Fellow of the Royal Astronomical Society in 1902. He died on June 29 last.

We hear also with regret of the death, which happened as long ago as October last, of Col. KNIGHT, of the Observatory, Harestock, Winchester. This gentleman was much interested in meteorology, and made observations of underground temperature.

Mr. G. C. BOMPAS, an amateur astronomer and a Member of the Royal Astronomical Society since 1894 December, died in June last.

THE TOTAL SOLAR ECLIPSE.—As is remarked in a note on page 332, astronomers are already on their way to their eclipse stations. We have had the pleasure lately of seeing Messrs. Miller and Cogshall, of the Kirkwood Observatory, at Greenwich, on their way through England, and of M. Greve we wrote in our last number. The Astronomer Royal and Mr. Dyson, with Prof. Sampson, have started for Sfax. Prof. Turner and Mr. Bellamy have left for Egypt. There is a rumour that Sir Norman and Dr. Lockyer with their party intended to have anchored the gunboat which is put at their disposal in the harbour of Philippeville, in Algeria, but for certain reasons the French object, although in a perfectly friendly spirit, to the presence just now of a ship of war of a foreign power in their waters, so the South Kensington official expedition will probably go to Palma, in Majorca. Several French observing-parties are arranged: MM. Deslandres and Rayet will be at Burgos; M. André, of the Lyons Observatory, at Tortosa; M. Trépied, of Algiers, with MM. Stephan and Borelly, of Marseilles, are going to Guelma, where there will be some observers from Paris; M. Bigourdan, of the Paris Observatory, proposes to make actinometric observations at Sfax, and there will also be a French expedition to Cistierna, in Leon. The Peninsular and Oriental Company are arranging to send a vessel by way of Gibraltar to Palma, where she will wait during the eclipse, and thence to Marseilles.

Dr. Johnstone Stoney points out in *Nature* of July 13 that, as Mercury at the time of the eclipse will be only 4° 6′ distant from the Sun's centre—about 2° 54′ south and 2° 54′ preceding—the illuminated part of Mercury will be an excessively thin crescent, which, if Mercury have an atmosphere, will have its horns prolonged by atmospheric refraction; so that if an observer is provided with a telescope on which a power of 200 can be used without loss of definition, he might see if the horns of Mercury are prolonged, and perhaps a spectrum of the tips of the crescent might be seen. If the whole of this programme can be carried out, we should find out whether Mercury has an atmosphere, and possibly learn something as to the constituents of the atmosphere.

Another line of observation, rather out of the common as an eclipse research, is called attention to by Mr. C. E. Stromeyer in the Journal of the B. A. A. for June 22. It may appear strange at first sight that a solar eclipse can help geodesy, but Mr. Stromeyer points out that as the path of the shadow is computed on the assumption that the Earth has a certain shape—that the equator is circular, for instance—if the observed path does not agree with the computed we may find corrections to our geodetical data. It is proposed to follow out this idea in Egypt, where it is hoped to arrange soldiers in a line or lines north and south across the track of the edge of the shadow, who will look at the Sun to see if it is totally eclipsed, and if one man sees the disk quite covered, whilst the next man north does not, the edge of the shadow-path will be fixed with some precision.

In the same number of the B. A. A. Journal, Mr. E. W. Maunder published a revised edition of "Eclipse Suggestions," which we have not space to quote at much length, but one important suggestion to observers who propose to study or draw the corona by the help of a telescope is that they should not try too much, but should devote themselves to some particular region, either (1) round one of the solar poles, (2) the neighbourhood of a bright prominence, or (3) at the root of some coronal streamer. A grating attached to one of the object-glasses of a pair of binoculars will serve as an efficient and convenient spectroscope. Messrs. T. Thorp & Co., of Whitefield, Manchester, are experts at arranging this.

The Paris Bureau des Longitudes have published a large scale map of the whole of Europe showing the path of totality and lines of equal observation, which can be bought from MM. Gautier-Villars for 2·50 fr. Part of this map showing the path of totality only across Europe is given in the 'Bulletin' of the Société Astronomique de France for July. The Observatory at Hamburg has issued a large diagram showing the stars round the Sun at time of eclipse.

THE BRITISH ASSOCIATION MEETING.—The Address of the President, Prof. G. H. Darwin, will be delivered at Cape Town on August 15, and at Johannesburg on August 30. Remembering the character of Prof. Darwin's particular researches, this will no doubt be of interest to our readers, and we hope to have an adequate abstract of this in our next number. Sir David Gill, as President of Section A (Mathematics and Physics), will take for the subject of his Address the geodetic work in South Africa. In the same Section Prof. Larmor will speak on the possible effect of the ether on the motion of matter, whilst Mr. A. R. Hinks will deliver at Bloemfontein one of the series of popular lectures, with the title "The Milky Way and the Clouds of Magellan."

ROYAL ASTRONOMICAL SOCIETY OF CANADA.—In the volume of Proceedings of this Society for the year 1904, recently received, we read with regret of the death of a former President whose name is familiar to us—Mr. Arthur Harvey, born 1834 April 23, died 1905 April 7, as we learn from the legend below his portrait, which is given as frontispiece. Curiously, the book opens with some paragraphs in which his name appears, obviously written before his death. The writer of these states that in every important particular Mr. Harvey had anticipated Mr. Maunder in his recent paper on the connection between the Sun and Earth-magnetism—that is, Mr. Harvey had shown that magnetic storms took place at intervals corresponding to a synodic rotation of the Sun, and had suggested that they were due to radiation of force of some kind from the Sun, but not necessarily in a single direct line from Sun to Earth. There seems to be little doubt that these ideas must have occurred to many persons, but Mr. Maunder saw the opportunity of proving the fact. The Canadian Society have done Mr. Maunder the honour of publishing a revised copy of his paper from the 'Monthly Notices' in this volume. Besides this, there are many other papers of the type characteristic of this Society, all well worth reading.

ASTRONOMY IN SHAKESPEARE.—The following appeared in a note on this subject in *T. P.'s Weekly* of June 2:—

> My second example is from 'Othello,' Act V. Scene 2, where the Moor, having murdered Desdemona, overwhelmed with dismay by an apparently relentless fate, exclaims, "Methinks it should be now a huge eclipse of sun and moon." I once twitted my friend, Mr. W. T. Lynn, F.R.A.S., with the fact that in his little work on "Remarkable Eclipses" I could not find any allusion to events that might explain this reference. My painstaking and erudite friend, however, very soon convinced me that, keeping in mind the date of the play, and the change in the Calendar, he could fix from his records with tolerable certainty the very phenomena which Shakespeare had in his mind when he penned those lines.

On showing this to Mr. Lynn, he said that he could not recall the author of this paragraph, but he remembered having once said that if Shakespeare was referring to some actual eclipse it was probably that of 1605 Oct. 12 which is mentioned in his book (p. 17).

HALLEY AND HIS COMET.—Mr. McPike, whose letter we publish in this number, is evidently interesting himself greatly in the comet whose orbit Dr. Halley computed with such success that it has ever since been associated with his name, and whose return is expected in 1910. We have lately received a short bibliography by Mr. McPike, giving titles of thirty-four works relating to this subject, and at his instigation the Boston Book Company have sent us their 'Bulletin of Bibliography' for July, which contains a long list of works forming material for a bibliography of the second Astronomer Royal.

SUN-SPOTS AND VEGETATION.—M. Flammarion has been making a study of the dates of first flowering and of the early appearances of the leaves on the chestnut-trees near his home in Paris. The tabulation of these dates since 1886 will be found in the 'Bulletin' of the Société Astronomique de France for July. After some smoothing of figures, the date curve shows considerable resemblance to the sun-spot curve—the leaves and flowers appearing earlier in years when the spots are at a maximum.

THE TIME OF INDIA.—A note in the *Times* states that the Government of India have ordered the introduction of a Standard

Time from 1905, July 1, on the railways, except on small local lines, where the change might be inconvenient, and in all telegraph offices in India and Burma. This standard time for India is to be that of the meridian 5½ hours east of Greenwich, and for Burma that of the meridian 6½ hours east. Hitherto, Madras time has been used on railways, and it has been convenient in most places to use railway time; but some of the large centres—Calcutta, Bombay, and Karachi—have each kept their own local time, and the Government do not prescribe the new standard time for these places, but if the local feeling of these places is in favour of the adoption of the new time the Government will be prepared to support the movement.

ERRATUM IN THE JULY NUMBER.—P. 294, line 31, *for* 40°·8 *read* 49°·8. No doubt readers have already realized that something was wrong here, for a mean temperature of 40°·8 at Greenwich would have been remarkable.

PROF. W. J. HUSSEY has resigned his position on the Lick staff, to take effect on 1905, October 1, in order to accept appointment as Professor of Astronomy in the University of Michigan and Director of the Detroit Observatory. The Regents of the University of California, in accepting his resignation, called attention to his faithful and efficient services at Lick, and mentioned specially his discovery of thirteen hundred double stars.

MR. PERCIVAL LOWELL has recently established a Fellowship—to be known as the Lawrence Fellowship—for the Department of Astronomy at Indiana University, Bloomington, Indiana, which carries with it, in addition to a liberal financial allowance, an opportunity for astronomical research in Lowell Observatory. Mr. John C. Duncan has been appointed Lawrence Fellow for 1905–6.

DR. R. H. CURTISS, who has been for some years a Fellow and lately Carnegie Assistant in the Lick Observatory, has been appointed Assistant Astronomer in the Allegheny Observatory.

PROF. C. A. YOUNG has retired from the Chair of Astronomy at Princetown University, which he has occupied for twenty-eight years.

FROM AN OXFORD NOTE-BOOK.

THERE is a considerable scattering of astronomers taking place just now. The eclipse track of 1905 is such as to suggest a number of independent expeditions—to Labrador, to Spain, to Algiers, to Tunis, to Egypt, and finally to somewhere on the ocean part of the track, where those expedited will be on board ship and need not make any tiresome astronomical observations; while over and above these there is the visit of the British Association to South Africa: so that half the astronomers in England, if not more, seem to be leaving it during this month of July. May they all have good weather, use it to advantage, and return safely to be present at the Solar Conference at Oxford on September 27!

It is rather warm weather for the work of preparation, and doubtless a number of compromises will be made at the last moment under the stress of the heat. Which reminds me of a story I heard lately about a foreign astronomer who landed in the United States with a very small knowledge of English and a consequently large anxiety about finding his way. He was delighted at his good fortune in meeting with a compatriot on the boat who had previously been in the States, and had a fluent acquaintance with the tongue spoken; and he readily accepted his proffered help. The first thing to do was to find a lodging, and the man of experience declared that the proper price was a dollar a day, and that they would find a respectable hotel at this price with a little perseverance. So they took up their hand-baggage and sallied forth. The first hotel interrogated named a dollar and a half as the lowest terms, and the guide immediately turned away. The weather was warm for carrying even hand-baggage, but the astronomer hoped for better luck the next time. Unfortunately, the lowest figure again turned out to be a dollar and a half; and the journey was resumed, the weather seeming to become, if anything, warmer still. Several hotels were visited with the same ill success, and the heat became oppressive. Finally the astronomer, to avoid further carrying of hand-bags in the enormous heat, proposed to accept the most recent offer of 3 dollars for the two of them, himself paying two of the dollars and his guide one. The offer was accepted with such alacrity as to suggest that it had been anticipated, and the astronomer has since been trying to determine whether it was really to his advantage to meet a compatriot who had been educated in the States.

The preparations for a new journey naturally recall with special freshness our delightful visit to the States last year, and perhaps that is why the above story came into my head so readily. It is certainly a wonderful journey from Atlantic to Pacific, or the other way. I suppose Profs. Campbell, Perrine, Hussey, and others are just about making it now on their way to the eclipse, and Prof. Hale will do so shortly to attend the Solar Conference. There are also several ladies on their way, one of whom becomes transformed into Mrs. Perrine on the Atlantic Coast. How are we to adapt the proverb about "The bride the Sun shines on" to the circumstances of a bride who takes a Total Solar Eclipse on the honeymoon?

WHEN Robert Louis Stevenson took that great journey "Across the Plains," he went as an emigrant and became seriously ill on the way. And he comforted himself by reflecting how much worse things were a generation earlier, when he would have had to go by waggon in fear of his life from Indians. One reads his words with new interest after taking the journey. I am tempted to quote a passage from his vivid sketch, partly to ask if anyone can suggest why he should have spoken of *one quarter* of the universe when he clearly means a half. He is saying that, however surprising we may find the courage of those who "crossed the plains" in early times, the early *settler* is a much more astonishing phenomenon:—

> A sky full of stars is the most varied spectacle that he can hope. He may walk five miles and see nothing; ten, and it is as though he had not moved; twenty, and still he is in the midst of the same great level, and has approached no nearer to the one object within view, the flat horizon which keeps pace with his advance. We are full at home of the question of agreeable wall-papers, and wise people are of opinion that the temper may be quieted by sedative surroundings. But what is to be said of the Nebraskan settler? His is a wall-paper with a vengeance—one quarter of the universe laid bare in all its gauntness. His eye must embrace at every glance the whole seeming concave of the visible world; it quails before so vast an outlook, it is tortured by distance: yet there is no rest or shelter till the man runs into his cabin, and can repose his sight upon things near at hand.

IN the general scattering of expeditions my own destination is Aswan in Egypt. It will doubtless be hot—the present weather is good practice—but we hope it will be fine. The instruments have started: we saw them on board a ship which was being loaded with boxes labelled "dynamite." We must confess the labels startled us at first, but we presently reflected that we could have few better guarantees for the careful handling of the ship. Capt. Lyons, R.E., is making all sorts of arrangements for our comfort and that of the American party, and others in Egypt seem to be ready to aid him in every way; but perhaps I had better quote from the *Journal du Caire* of June 24:—

> Assouan qui a déjà bénéficié des gloires d'un barrage fameux est la ville désignée aux rendez-vous des astronomes.
>
> Ils n'y manqueront pas. L'astronome, qui, à la contemplation des astres étients, a gagné une certaine insensibilité aux feux de la terre, n'est pas homme à s'effrayer de nos températures soudanaises.
>
> Nous pouvons donc être sûrs que notre mois d'août, si vide d'ordinaire, ramènera cette année une saison nouvelle, saison estivale et scientifique.
>
> Déjà sont annoncées trois missions: mission anglaise, mission américaine, mission russe. J'ose espérer que nos plus clairvoyants télescopes français seront également représentés. Et quant aux Japonais, je parierais volontiers trois mois de congé en Europe, que les petits tracas de Mandchourie ne les empêcheront pas de montrer que l'empire du soleil levant ne saurait se désintéresser du soleil éclipsé. Comme nous ne sommes plus au temps des superstitions, je ne pense pas qu'ils aillent voir là un funeste présage. Il est donc probable qu'aux noms de M. Turner, professeur d'astronomie à Oxford, de Mme Bellamy de l'Observatoire de Greenwich, de M. W. W. Campbell, de New-York, viendront s'ajouter quelques autres noms en KiKi, KoKo, etc.

Préparons à ces visiteurs une réception digne de leur courage. Le mieux sera de leur faire escorte jusqu'à Assouan.

C'est aux dames cairotes surtout que ce devoir incombe. Rien ne sera plus doux aux savants étrangers que l'exquis parfum de leur présence. Astronomie et galanterie sont sœurs et la vision des formes solaires, lunaires, planétaires, de toutes les formes célestes, n'émousse en rien la sensibilité à l'harmonieuse caresse des formes terrestres. Le télescope n'a point de patrie; il n'a point d'âge non plus.

As already stated, we hope it will be fine, and we have every assurance that it will be, save one only. A warning was uttered the other day that since the new dam and irrigation works, the climate of Egypt was quite changed and that rain and cloud were becoming familiar. If we are favoured with cloud on August 30 it will add one more to an extraordinary series of acts on the part of the weather to show its contempt for forecasts. Many of them have been mentioned in these notes already, but until recently I suppose that the weather had specially singled out total eclipses for a display of malice, and did not know that transits of Venus had been treated in the same way. The following is from Prof. Newcomb's 'Reminiscences of an Astronomer,' p. 172:—

> What made this result more tantalizing was that the very pains we had taken to collect the data proved, by chance, to have made the choice worse. For some time it was deliberated whether the Japanese station should be in Nagasaki or Yokohama. Consultation with the best authorities and a study of the records showed that, while Yokohama was a favorable spot, the chances were somewhat better at Nagasaki. So to Nagasaki the party was sent. But when the transit came, while the sky was of the best at Yokohama, it was far from being so at Nagasaki.
>
> Something of the same sort occurred at the most stormy of all the southern stations, that at Kerguelen Island. The British expeditions had, in the beginning, selected a station on this island known as Christmas Harbor.
>
> We learned that a firm of New London, Conn., had a whaling station on the island. It was therefore applied to to know what the weather chances were at various points in the island. Information was obtained from their men, and it was thus found that Molloy Point, bad though the weather there was, afforded better chances than Christmas Harbor, so it was chosen. But this was not all; the British parties, either in consequence of the information we had acquired or through what was learned from the voyage of the 'Challenger,' established their principal station near ours. But it happened that the day at Christmas Harbor was excellent, while the observations were greatly interfered with by passing clouds at Molloy Point.

THERE are, I imagine, few of us who have gone through the work necessary for an eclipse expedition without an occasional doubt, born of weariness, whether the end is worth all the trouble. It is but a passing doubt, I hope, and our better judgment soon returns. But I remember feeling grateful to Prof. Campbell a year ago for putting down some clear strong words on the subject, and I shall venture to reproduce them here:—

> The influence of eclipse observations upon solar and astrophysical research has been most remarkable. The reversing layer, the chromosphere, the prominences, and the corona were in fact discovered at eclipses. Many of our present

everyday methods of studying them are also eclipse products. The richness of eclipse results, considering the remarkably short intervals available for observation, is unique in science. To realize this, we need only recall that the durations of observable total eclipses, clear and cloudy, have amounted altogether to about one hour since the spectroscope was applied to the problem, and about half an hour since photographic methods have prevailed. (*Popular Science Monthly*, June 1904.)

BULLETIN No. 21 of the Lowell Observatory contains prints of the photographs of which it is claimed that they show the canals of Mars. I do not mean to deny the claim any more than I am prepared to admit it. Personally I find it extremely difficult to say exactly what is on those prints, and friends to whom I have shown them differ a good deal in their interpretations. I am glad to hear that a drawing from these photographs and a note by Mr. Wesley, who has had so much experience in studying photographs, is to be published in the number in which this will appear.

BY the way, Mr. Crommelin remarked a year ago that in May last the Earth and Moon would appear to transit the Sun, as seen from Mars (Mon. Not. lxiv. p. 520), a phenomenon which occurred last in 1879, and will not occur again till 1984. I wonder whether anyone on Earth took the opportunity, as suggested by Mr. Crommelin, to measure the diameter and polar compression with absolute symmetry of illumination round the limb of the planet. I suppose the Martians can get the parallax of minor planets so easily that the occasion would be of little use to them.

THE old use of the word equation in the phase "personal equation" is too obvious a stumbling-block not to catch people every now and then. The *Daily Telegraph* in commenting on the prospects of a Radical Government at an early date said:—

> There are also "personal equations" to be solved, which can be ignored in Opposition, but which would obtrude themselves in the work of Cabinet-making.

OUR friend the *Globe* produced the following yesterday (July 21):—

ASTRAL REFRACTION.

> The atmosphere through which we view the heavenly bodies being curved like a watch glass, causes a certain deflection in their apparent line of direction as observed from the Earth, and introduces a certain difficulty into the study of their laws of motion. M. Loeury has just laid before the French Academy of Sciences a device perfected by himself for correcting this deviation. Selecting two stars near enough to be viewed through one lens, he takes an exact measurement by means of two mirrors held in the hand like the two branches of a compass of the angle which separates them. This angle is, of course, always the same, since the two stars always respond in parallel degree to the apparent movement of the celestial sphere. Any difference in the angle is, therefore, that of the atmospheric refraction.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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Meeting of the British Association in South Africa, 1905.— Address by the President, Prof. G. H. DARWIN, M.A., F.R.S.*

Cosmical Evolution.

THE problems of cosmical evolution are so complicated that it is well to conduct the attack in various ways at the same time. Although the several theories may seem to some extent discordant with one another, yet, as I have already said, we ought not to scruple to carry each to its logical conclusion. We may be confident that in time the false will be eliminated from each theory, and when the true alone remains the reconciliation of apparent disagreements will have become obvious.

The German astronomer Bode long ago propounded a simple empirical law concerning the distances at which the several planets move about the Sun. It is true that the planet Neptune, discovered subsequently, was found to be considerably out of the place which would be assigned to it by Bode's law, yet his formula embraces so large a number of cases with accuracy that we are compelled to believe that it arises in some manner from the primitive conditions of the planetary system.

The explanation of the causes which have led to this simple law as to the planetary distances presents an interesting problem, and, although it is still unsolved, we may obtain some insight into its meaning by considering what I have called a working model of ideal simplicity.

Imagine then a sun round which there moves in a circle a single large planet. I will call this planet Jove, because it may be taken as a representative of our largest planet, Jupiter. Suppose next that a meteoric stone or small planet is projected in any perfectly arbitrary manner in the same plane in which Jove is moving; then we ask how this third body will move. The conditions imposed may seem simple, yet the problem has so far overtaxed the powers of the mathematician that nothing approaching a general answer to our question has yet been given. We know, however, that under the combined attractions of the Sun and Jove the meteoric stone will in general describe an orbit of extraordinary complexity, at one time moving slowly at a great distance from both the Sun and Jove, at other times rushing close past one or other of them. As it grazes past Jove or the Sun it may often but just escape a catastrophe, but a time will come at length when it runs its chances too fine and comes into actual collision. The individual career of the stone is then ended by absorption, and of course by far the greater chance is that it will find its Nirvana by absorption in the Sun.

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* Prof. Darwin delivered the first part of his address at Capetown on August 15, and the second at Johannesburg on August 30. The first part of this treated generally of evolution and of the recently propounded corpuscular theory. The second, which deals with subjects of astronomical interest, with Prof. Darwin's kind permission is here given.

Next let us suppose that instead of one wandering meteoric stone or minor planet there are hundreds of them, moving initially in all conceivable directions. Since they are all supposed to be very small, their mutual attractions will be insignificant, and they will each move almost as though they were influenced only by the Sun and Jove. Most of these stones will be absorbed by the Sun, and the minority will collide with Jove.

When we inquire how long the career of a stone may be, we find that it depends on the direction and speed with which it is started, and that by proper adjustment the delay of the final catastrophe may be made as long as we please. Thus by making the delay indefinitely long we reach the conception of a meteoric stone which moves so as never to come into collision with either body.

There are, therefore, certain perpetual orbits in which a meteoric stone or minor planet may move for ever without collision. But when such an immortal career has been discovered for our minor planet, it still remains to discover whether the slightest possible departure from the prescribed orbit will become greater and greater and ultimately lead to a collision with the Sun or Jove, or whether the body will travel so as to cross and recross the exact perpetual orbit, always remaining close to it. If the slightest departure inevitably increases as time goes on, the orbit is unstable; if, on the other hand, it only leads to a slight waviness in the path described, it is stable.

We thus arrive at another distinction: there are perpetual orbits, but some, and indeed most, are unstable, and these do not offer an immortal career for a meteoric stone; and there are other perpetual orbits which are stable or persistent. The unstable ones are those which succumb in the struggle for life, and the stable ones are the species adapted to their environment.

If, then, we are given a system of a sun and large planet, together with a swarm of small bodies moving in all sorts of ways, the sun and planet will grow by accretion, gradually sweeping up the dust and rubbish of the system, and there will survive a number of small planets and satellites moving in certain definite paths. The final outcome will be an orderly planetary system in which the various orbits are arranged according to some definite law.

But the problem presented even by a system of such ideal simplicity is still far from having received a complete solution. No general plan for determining perpetual orbits has yet been discovered, and the task of discriminating the stable from the unstable is arduous. But a beginning has been made in the determination of some of the zones surrounding the Sun and Jove in which stable orbits are possible, and others in which they are impossible. There is hardly room for doubt that if a complete solution for our solar system were attainable, we should find that the orbits of the existing planets and satellites are numbered amongst the stable perpetual orbits, and should thus obtain a rigorous mechanical explanation of Bode's law concerning the planetary distances.

It is impossible not to be struck by the general similarity between the problem presented by the corpuscles moving in orbits in the atom, and that of the planets and satellites moving in a planetary system. It may not, perhaps, be fanciful to imagine that some general mathematical method devised for solving a problem of cosmical evolution may find another application to miniature atomic systems, and may thus lead onward to vast developments of industrial mechanics. Science, however diverse its aims, is a whole, and men of science do well to impress on the captains of industry that they should not look askance on those branches of investigation which may seem for the moment far beyond any possibility of practical utility.

You will remember that I discussed the question as to whether the atomic communities of corpuscles could be regarded as absolutely eternal, and that I said that the analogy of other moving systems pointed to their ultimate mortality. Now the chief analogy which I had in my mind was that of a planetary system.

The orbits of which I have spoken are only perpetual when the bodies are infinitesimal in mass, and meet with no resistance as they move. Now the infinitesimal body does not exist, and both Lord Kelvin and Poincaré concur in holding that disturbance will ultimately creep in to any system of bodies moving even in so-called stable orbits; and this is so even apart from the resistance offered to the moving bodies by any residual gas there may be scattered through space. The stability is therefore only relative, and a planetary system contains the seeds of its own destruction. But this ultimate fate need not disturb us either practically or theoretically, for the solar system contains in itself other seeds of decay which will probably bear fruit long before the occurrence of any serious disturbance of the kind of which I speak.

Before passing on to a new topic I wish to pay a tribute to the men to whom we owe the recent great advances in theoretical dynamical astronomy. As treated by the master-hands of Lagrange and Laplace and their successors, this branch of science hardly seemed to afford scope for any great new departure. But that there is always room for discovery, even in the most frequented paths of knowledge, was illustrated when, nearly thirty years ago, Hill, of Washington, proposed a new method of treating the theory of the Moon's motion in a series of papers which have become classical. I have not time to speak of the enormous labour and great skill involved in the completion of Hill's Lunar Theory, by Ernest Brown, whom I am glad to number amongst my pupils and friends; for I must confine myself to other aspects of Hill's work.

The title of Hill's most fundamental paper, namely, "On Part of the Motion of the Lunar Perigee," is almost comic in its modesty, for who would suspect that it contains the essential points involved in the determination of perpetual orbits and their stability? Probably Hill himself did not fully realise at the time the full importance of what he had done. Fortunately he was followed by Poincaré, who not only saw its full meaning, but devoted his incomparable mathematical powers to the full theoretical development of the point of view I have been laying before you.

Other mathematicians have also made contributions to this line of investigation, amongst whom I may number my friend Mr. Hough, chief assistant at the Royal Observatory of Cape Town, and myself. But without the work of our two great forerunners we should still be in utter darkness, and it would have been impossible to give even this slight sketch of a great subject.

The theory which I have now explained points to the origin of the Sun and planets from gradual accretions of meteoric stones, and it makes no claim to carry the story back behind the time when there was already a central condensation or sun about which there circled another condensation or planet. But more than a century ago an attempt had already been made to reconstruct the history back to a yet remoter past, and, as we shall see, this attempt was based upon quite a different supposition as to the constitution of the primitive solar system. I myself believe that the theory I have just explained, as well as that to which I am coming, contains essential elements of truth, and that the apparent discordances will some day be reconciled. The theory of which I speak is the celebrated Nebular Hypothesis, first suggested by the German philosopher Kant, and later restated independently and in better form by the French mathematician Laplace.

Laplace traced the origin of the solar system to a nebula or cloud of rarefied gas congregated round a central condensation which was ultimately to form the Sun. The whole was slowly rotating about an axis through its centre, and, under the combined influences of rotation and of the mutual attraction of the gas, it assumed a globular form, slightly flattened at the poles. The justifiability of this supposition is confirmed by the observations of astronomers, for they find in the heavens many nebulæ, while the spectroscope proves that their light at any rate is derived from gas. The primeval globular nebula is undoubtedly a stable or persistent figure, and thus Laplace's hypothesis conforms to the general laws which I have attempted to lay down.

The nebula must have gradually cooled by radiation into space, and as it did so the gas must necessarily have lost some of its spring or elasticity. This loss of power of resistance then permitted the gas to crowd more closely towards the central condensation, so that the nebula contracted. The contraction led to two results, both inevitable according to the laws of mechanics: first, the central condensation became hotter; and, secondly, the speed of its rotation became faster. The accelerated rotation led to an increase in the amount of polar flattening, and the nebula at length assumed the form of a lens, or of a disk thicker in the middle than at the edges. Assuming the existence of the primitive nebula, the hypothesis may be accepted thus far as practically certain.

From this point, however, doubt and difficulty enter into the argument. It is supposed that the nebula became so much flattened that it could not subsist as a continuous aggregation of gas, and a ring of matter detached itself from the equatorial regions. The central portions of the nebula, when relieved of the excrescence, resumed the more rounded shape formerly possessed by the whole. As the cooling continued the central portion in its turn became excessively flattened through the influence of its increased rotation; another equatorial ring then detached itself, and the whole process was repeated as before. In this way the whole nebula was fissured into a number of rings surrounding the central condensation, whose temperature must by then have reached incandescence.

Each ring then aggregated itself round some nucleus which happened to exist in its circumference, and so formed a subordinate nebula. Passing through a series of transformations, like its parent, this nebula was finally replaced by a planet with attendant satellites.

The whole process forms a majestic picture of the history of our system. But the mechanical conditions of a rotating nebula are too complex to admit, as yet, of complete mathematical treatment, and thus, in discussing this theory, the physicist is compelled in great measure to adopt the qualitative methods of the biologist, rather than the quantitative ones which he would prefer.

The telescope seems to confirm the general correctness of Laplace's hypothesis. Thus, for example, the great nebula in Andromeda presents a grand illustration of what we may take to be a planetary system in course of formation. In it we see the central condensation surrounded by a more or less ring-like nebulosity, and in one of the rings there appears to be a subordinate condensation.

Nevertheless it is hardly too much to say that every stage in the supposed process presents to us some difficulty or impossibility. Thus we ask whether a mass of gas of almost inconceivable tenuity can really rotate all in one piece, and whether it is not more probable that there would be a central whirlpool surrounded by more slowly-moving parts. Again, is there any sufficient reason to suppose that a series of intermittent efforts would lead to the detachment of distinct rings, and is not a continuous outflow of gas from the equator more probable?

The ring of Saturn seems to have suggested the theory to Laplace; but to take it as a model leads us straight to a quite fundamental difficulty. If a ring of matter ever concentrates under the influence of its mutual attraction, it can only do so round the centre of gravity of the whole ring. Therefore the matter forming an approximately uniform ring, if it concentrates at all, can only fall in on the parent planet and be reabsorbed. Some external force other than the mutual attraction of the matter forming the ring, and therefore not provided by the theory, seems necessary to effect the supposed concentration. The only way of avoiding this difficulty is to suppose the ring to be ill-balanced or lop-sided; in this case, provided the want of balance is pronounced enough, concentration will take place round a point inside the ring but outside the planet. Many writers assume that the present distances of the planets preserve the dimensions of the primitive rings; but the argument that a ring can only aggregate about its centre of gravity, which I do not recollect to have seen before, shows that such cannot be the case.

The concentration of an ill-balanced or broken ring on an interior point would necessarily generate a planet with direct rotation—that is to say, rotating in the same direction as the Earth. But several writers, and notably Faye, endeavour to show—erroneously, as I think—that a retrograde rotation should be normal, and they are therefore driven to make various complicated suppositions to explain the observed facts. But I do not claim to have removed the difficulty, only to have shifted it; for the satellites of Neptune, and presumably the planet itself, have retrograde rotations; and, lastly, the astonishing discovery has just been made by William Pickering of a ninth retrograde satellite of Saturn, while the rotations of the eight other satellites, of the ring and of the planet itself, are direct. Finally, I express a doubt as to whether the telescope does really exactly confirm the hypothesis of Laplace, for I imagine that what we see indicates a spiral rather than a ring-like division of nebulæ *.

This is not the time to pursue these considerations further, but enough has been said to show that the Nebular Hypothesis cannot be considered as a connected intelligible whole, however much of truth it may contain.

In the first theory which I sketched as to the origin of the Sun and planets, we supposed them to grow by the accretions of meteoric wanderers in space, and this hypothesis is apparently in fundamental disagreement with the conception of Laplace, who watches the transformations of a continuous gaseous nebula. Some years ago a method occurred to me by which these two discordant schemes of origin might perhaps be reconciled. A gas is not really continuous, but it consists of a vast number of molecules moving in all directions with great speed and frequently coming into collision with one another. Now I have ventured to suggest that a swarm of meteorites would, by frequent collisions, form a medium endowed with so much of the mechanical properties of a gas as would satisfy Laplace's conditions. If this is so, a nebula may be regarded as a quasi-gas, whose molecules are meteorites. The gaseous luminosity which undoubtedly is sent out by nebulæ would then be due only to incandescent gas generated by the clash of meteorites, while the dark bodies themselves would remain invisible. Sir Norman Lockyer finds spectroscopic evidence which led him long ago to some such view as this, and it is certainly of interest to find in his views a possible means of reconciling two apparently totally discordant theories †. However I do not desire to lay much stress on my suggestion, for without doubt a swarm of meteors could only maintain the mechanical properties of a gas for a limited time, and, as pointed out by Prof. Chamberlin, it is difficult to understand how a swarm of meteorites moving indiscriminately in every direction could ever have come into existence. But my paper may have served to some extent to suggest to Chamberlin his recent modification of the Nebular Hypothesis, in which he seeks to reconcile Laplace's view with a meteoritic origin of the planetary system ‡.

[To be continued.]

[MR. A. R. HINKS has promised to send us some short notes describing the more personal proceedings of the English Members interested in mathematical or astronomical subjects. A first instalment follows on page 344.—EDS.]

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* Professor Chamberlin, of Chicago, has recently proposed a modified form of the Nebular Hypothesis, in which he contends that the spiral form is normal. See 'Year Book,' No. 3, for 1904, of the Carnegie Institution of Washington, pp. 195–258.

† Newcomb considers the objections to Lockyer's theory insuperable. See p. 190 of 'The Stars,' John Murray, London, 1904.

‡ See preceding reference to Chamberlin's Paper.

The Astronomical Society of the Atlantic.

Early in the morning of July 26 a kind of sedan chair on runners, drawn by two bullocks, carried up through the town of Funchal, from the jetty to the foot of the mountain-railway, four astronomers. In a few minutes they were joined by a fifth, who had come in a tram drawn by three mules, and they all went up in the rack-railway to breakfast on the mountain at Reid's Hotel. There on the verandah, after breakfast, they constituted the Astronomical Society of the Atlantic, looking out over the ocean which lay pacific at their feet, and at the 'Kildonan Castle,' which was taking them south to the Meeting of the British Association at the Cape. The "protocol" of the Society concerns discussion of a scheme which one of the Members is presently to launch; its more informal proceedings will, if the Editors permit, be chronicled in these pages.

The first business of the Society was to elect a sixth Member, who got up too late for the inaugural breakfast; the second was to get down the mountain again in the exhilarating fashion peculiar to Madeira. Imagine a large and low basket-chair on greased runners, propelled violently down a steep, greasy, cobbled street by two Portuguese running behind and hooking the concern round corners with a pair of strings; that is how the Society made its first journey, down 2000 feet to the quay, and thence in a launch out to the ship through the crowds of boys diving for money, and boatloads of parrots and basket-chairs. Conversation turned to the probability of sharks catching the divers, and the President of the Society remarked: "When I was in Spitzbergen I saw also a shark." "Yes, but in the Museum," was the retort of the Member whose plans provided the Society with unfailing subjects for discussion during the rest of the voyage.

On a Form of Error in Eye-estimated Transits of Spots on Jupiter.

Some interesting and important papers have recently been communicated to the Royal Astronomical Society by Prof. G. W. Hough and Mr. A. S. Williams on the relative value of the two methods of determining longitudes on Jupiter by micrometric measures and eye-estimated transits of spots over the planet's central meridian. In the course of the discussion allusion has been made to Schmidt's observations of the Red Spot between 1879 and 1881, which appeared to exhibit a variable error possibly depending on the hour-angle of the planet.

I have recently discussed my own eye-estimated transit-observations of the Red Spot Hollow (the spot itself being frequently too faint and difficult to give satisfactory results except under the best atmospheric conditions) with a view to determine whether they are affected by a similar error to that suspected by Schmidt. The observations cover a period of seven apparitions and are about 140 in number.

The results of my discussion will be apparent from the accompanying table and diagram. To begin with, the observations of each separate apparition have been compared *inter se*, and the rotation-period which best satisfies the series deduced. The residuals are fairly small, the mean accidental error on the assumption of uniform periods being on the yearly average $\pm 2^{m}$. The third column in the table gives the rotation-periods determined in this way for the last seven apparitions of the planet. Next, for comparison with these figures I have taken the apparent longitude of the Hollow at the dates of successive "oppositions" and deduced the mean rotation-periods in the intervals between them. It will be seen that the values determined by this method (column 4) are slightly less ($0^{s}\cdot4$ on the average) than those found by a comparison of each year's observations *inter se*. Now, since it is impossible to suppose that the planet's rotation is systematically retarded during the months of its visibility and accelerated near the time of its conjunction with the Sun, I am forced to the conclusion that at the beginning of each apparition, when the planet's hour-angle is East, I observe transits of the object in question a little too early, and at the end of the apparition, when the hour-angle is West, I observe them a little too late. In other words, it seems certain that, in my case at any rate, there is an error (quite distinct from accidental error and a constant personal equation) depending on the time at which the observations are made.

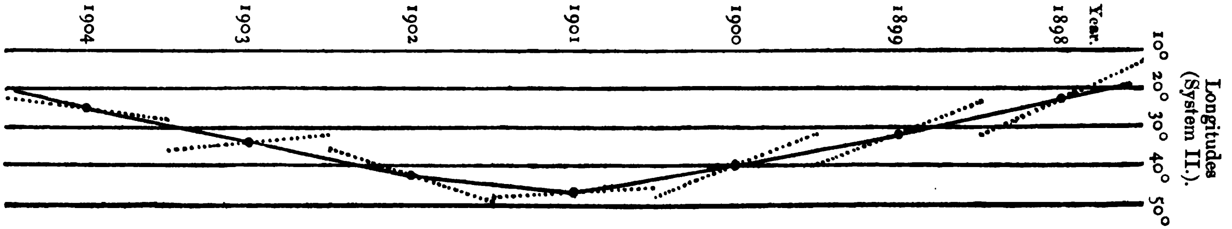
Perhaps the effect of the error will be more clearly seen by reference to the diagram. The dots give the longitude of the Hollow at the dates of "opposition." The line joining them shows the mean motion of the spot from year to year, and the dotted lines indicate the mean *rate* at which the object was drifting in longitude according to the separate observations of each apparition. The difference in the slant of the lines is readily apparent, and in nearly every case is of the same kind. In 1901 the Hollow exhibited a gradual change of rate, and as regards 1902 and 1904 the effect of the error is in the one case entirely and in the other partly masked on the chart by the undoubted temporary acceleration of the Red Spot in consequence of the passage round it of the great S. tropical disturbance.

If the error in question is due to the varying slant of the belts and the failure of the eye to estimate correctly the position of the line that bisects the disc and is truly normal to the planet's equator, its effect will, of course, depend on the latitude of the spots observed, and will be of opposite character in the two

Observations of the "Red Spot Hollow" on Jupiter.

Date of "Opposition."	Apparent longitude at "Opposition."	No. of rotations observed.	Deduced rotation-period.	Mean rotation-period between successive "Oppositions."
	λ. °		h m s	h m s
1898 March 25	22·3	480	9 55 42·4	9 55 41·6
1899 April 25	31·2	521	9 55 42·1	9 55 41·5
1900 May 27	40·0	557	9 55 42·1	9 55 41·3
1901 June 30	46·5	475	9 55 40·8	9 55 40·2
1902 August 5	42·0	695	9 55 39·3	9 55 39·8
1903 September 11	33·8	410	9 55 41·1	9 55 39·8
1904 October 18	25·3	287	9 55 40·2	
Means	......	489	9 55 41·1	9 55 40·7

Diagram showing Drift of Red Spot Hollow in Longitude.



hemispheres. I regret that I have not been able to test this matter, as spots in the N. hemisphere of a suitable latitude are so evanescent and so variable in motion that it is scarcely possible to be certain of their identification from year to year. Assuming the suggested explanation to be correct, the error should disappear if care is taken to keep the line joining the eyes always parallel to the belts.

As regards the bearing of this variable error on the value of eye-estimated transits, the following points seem worthy of notice:—

(1) The error arising from this cause in the rotation-period deduced from a comparison of a series of eye-estimated observations *inter se* is small. In my case it seems for the Red Spot Hollow to be just a little less than half a second. This is very much smaller than the figures given by Prof. Hough in M. N. vol. lxv. 1905 May, p. 684. In the instances he quotes the time-intervals are not very large—perhaps scarcely sufficient to entirely eliminate the effects of "accidental" error. It is possible that for spots near the equator the variable error may not exist at all.

(2) In my case the variation is apparently *systematic.* It is possible, therefore, that now the error is known it may with care be avoided, or at any rate a suitable correction applied for it.

(3) The errors in final results arising from "variable personal equation" are far less serious than those due to mistakes in the identification of spots. Except in the cases of the Red Spot and just one or two other unmistakable objects, a few observations, however accurate they may be in themselves, are of little or no value if separated by considerable intervals of time, since it is impossible to be certain that they relate at all to the same object. Results that are most erroneous have doubtless often been deduced in this way. On the other hand, a long and full series of eye-estimated transits which admits of no doubt as to the question of identification, even if sometimes the separate observations fall short of the accuracy possibly attainable in micrometric measures, is of very great value, and, as Mr. A. S. Williams has already pointed out (M. N. vol. lxiv. 1904 March, p. 429), positions determined by this method "constitute the foundation of much of our knowledge of the rotation of Jupiter and of the various surface-currents known to exist."

I think it might be well to put the result of my inquiry on record, and perhaps other observers may be induced thereby to throw some light on the matter from their own experience.

THEODORE E. R. PHILLIPS.

New Measurements of the Distance of the Sun *.

The problem of the determination of the distance of the Sun is, in some respects at least, the most fundamental in the whole range of astronomy, for the number which represents it is involved in almost any calculation of distances and masses, of sizes and densities, either of planets or their satellites or of the stars. The distance of the Sun bears somewhat the same relation to other problems of celestial surveying as the size and shape of the Earth bear to terrestrial. It may not always appear on the surface, but it is generally concealed somewhere in the depths of the calculations. And I am compelled to confess that in one respect the earth-measurers have the advantage over astronomers. The utmost that the astronomer can do is to show that the distance of the Sun is so many times the radius of the Earth. But ask him to put it into miles and he is powerless to do so until the geodesists have told him how large the Earth is; and it is there that, in the very nature of the case, we are compelled to depend in the end upon the scientific labours of your Corps.

Let us look at the matter for a moment as a problem in pure surveying. To measure the distance of the Sun we have as a base a chord somewhat less than the diameter of the Earth, since observations cannot be made on a heavenly body when it is actually on the horizon. Suppose we put our base line at $\frac{8}{10}$ of the diameter. Our problem is to determine the distance of a body so far away that the whole diameter of the Earth subtends at it an angle of only about 17·6 seconds of arc; and with our somewhat diminished base this angle is reduced to a little less than 15″. I believe that the length of your base upon the Great Lines of Chatham is about 1730 feet. Imagine that from that base you had to determine with an accuracy greater than one in a thousand the distance of an intersected point about 4500 miles away, as far away as Chicago; and you have a problem which is by comparison simplicity itself. For the ends of our 7000-mile base are not visible from each other, being on opposite sides of the world; and our angles at the base must be determined by a complicated reference to the zenith, with all the well-known impossibilities of determining absolute places in the sky increased by the special difficulties that arise when the object to be observed is the Sun. You will readily grant that to determine the distance of the Sun by direct observation of that body is impossible unless you are content with an accuracy of about 1 in 10.

Now it is a curious fact that there is a way of obtaining the distance of the Sun with an accuracy of 10 per cent. with no other instrument than a clock keeping accurate time; you do it by observing the times of minima of the variable star Algol. Every 2 days 21 hours Algol drops more than a magnitude, and does this with a regularity which would be unfailing were it not for the fact that at one season of the year we are nearer the star by nearly the whole diameter of the Earth's orbit than we are at the opposite season; and light takes about 16 minutes to traverse that distance. In the middle of November the eclipses of Algol are taking place 8 minutes earlier than the average; in May, could we observe the star so near the Sun, they would be found 8 minutes behind their time; and a practised observer could, on a long series of observations, determine that inequality, with a total range of 16 minutes, well within 2 minutes, that is to say, with an accuracy of about 10 per cent. We have then only to combine this quantity with the known velocity of light, and we have a measure of the Sun's distance. A mere curiosity in itself, it will serve to introduce us to some indirect ways of determining the distance of the Sun which have, both practically and historically, a peculiar interest and importance.

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* Lecture delivered at the Royal Engineers Institute on 9th February, 1905, by A. R. Hinks, M.A. Reprinted by permission from the 'Royal Engineers' Journal' for 1905 July.

At the present time we are in the thick of a new determination of the distance of the Sun on a scale of operations greater than has been known before. More than 50 observatories of the Northern hemisphere are engaged more or less deeply on the work, which has occupied a great many of us closely for the last four years, and will give plenty of trouble to some of us for several years to come. Before we enter upon the consideration of the new method and the new opportunities, we might well pause to answer the question, which is by no means superfluous,—How does it come about that, at the end of the 19th century, which had seen attempts almost innumerable to measure the distance of the Sun, the result was still so much in doubt that it was worth while to concentrate quite a large proportion of the total astronomical energy of the world upon a new attempt? I believe that we shall find some explanation of this fact if we examine the history of the various values of the Solar Parallax that were used in the *Nautical Almanac* during the 19th century.

A determination of the distance of the Sun by direct observation of the Sun itself is impracticable; the Sun is too difficult an object to observe with any great accuracy; its distance is too great, and our base is too small, for any method of direct trigonometrical survey to be possible. But we can in effect diminish its distance by substituting for it one of the planets, which can be more accurately observed; for when the distance of any one planet from the Earth is known, the dimensions of other orbits follow by the application of Kepler's 3rd law. And at the same time we can, as we shall see, secure the inestimable advantage that the measures to be made are relative and not absolute.

Let me digress for a moment to insist upon the importance of this distinction. If you wished to find the difference in latitude and longitude between your Institute and the trigonometrical point at Darland, you might determine the latitude and longitude of each, and take the differences; or you might triangulate from one to the other. One is an absolute method, the other a relative; and it is scarcely necessary to emphasize the difference in accuracy between the two. We shall see that, various as are the kinds of measurement which may be made to contribute to a knowledge of the solar parallax, they are all of them relative measurements. For example, one may observe the displacement of the planet Mars among the stars, as seen from a northern and a southern station, say Greenwich and the Cape; or one may observe the displacement of the place of Venus in transit across the Sun from stations suitably chosen. In each case we are measuring the displacement as viewed from different stations of a near object with respect to one further off, the displacement of Mars among the stars or of Venus against the Sun. We have secured the advantages that the parallactic displacement to be measured is greater than that of the Sun itself; that the objects to be observed, Mars or Venus, are better adapted for observation; and that the measures are relative.

In the middle of the 18th century Lacaille made observations of Mars at the Cape of Good Hope, which were compared with others made at various observatories in Europe; and he deduced a parallax of about 10″. In the same century there occurred the two famous transits of Venus of 1761 and 1769, which were very extensively observed, among others by Captain Cook on his celebrated expedition for that purpose to the South Seas. Many and various were the results obtained by different discussions of the observations, lying between 8½″ and 9½″, but decidedly less than the parallax found from Mars; and we find that at the beginning of the 19th century the *Nautical Almanac* adopted a value in round numbers, 9″, as the best that could be made of them.

Values of the Solar Parallax used in the *Nautical Almanac* during the 19th Century.

	π.
1801–1833	9″
1834–1869	8·5776
Encke, from Transits of Venus, 1761 and 1769.	
1870–1881	8·95
Leverrier, from Parallactic Inequality of Moon (1858).	
1882–1900	8·848
Newcomb, from general mean of many methods (1867).	

In 1824 the German astronomer Encke submitted to a very searching examination the collected results of the transit of 1769, and deduced the result 8″·5776, which, with its imposing train of decimals intact, was incorporated in our *Nautical Almanac* for 1834, survived until 1869, and was responsible for the statement, which many of us can remember in the school books, that the distance of the Sun is 95,000,000 miles.

Meanwhile the attack upon the problem had been maintained in several different ways, and particularly by an indirect method that has many points of interest.

In the Lunar Theory there occurs, among the short-period perturbations to which the motion of the Moon is subject, an inequality in a period of a month which depends upon the fact that the disturbing action of the Sun is greater on that half of the Moon's orbit which lies towards the Sun than upon the other half. The result of this is that the Moon is more than 2 minutes behind at first quarter and 2 minutes ahead at last quarter of the place which she would occupy were there no perturbation. It is clear that the magnitude of the effect must depend upon the ratio of the distances of the Sun and Moon from the Earth; and since the effect is large, an oscillation either way of about 125″, this should give a strong determination of the solar parallax, provided that the Moon can be observed with the required accuracy and that the theoretical relation between the perturbation and the solar parallax is firmly established. In 1858 Leverrier found in this way a value of 8″·95; several other determinations supported this large value; and practically all the determinations made since 1830, however much they might disagree among themselves, agreed at any rate in one thing, that Encke's value was much too small. We find, therefore, that in the *Nautical Almanac* for 1870, published in 1866, Leverrier's value 8″·95 is adopted, and the official distance of the Sun changed at one swoop from 95,000,000 to 91,000,000 miles.

But now preparations were in full swing for the observations of the Transits of Venus of 1874 and 1882, which for many years had been eagerly awaited in the full expectation and belief that then, with all the manifold improvements in the arts of observation, in the invention of the heliometer and the application of photography to celestial measurement, the question of the solar parallax would be definitely settled. We cannot do more than glance at the most beautiful and most complicated geometrical problems involved in the consideration of all the circumstances of a Transit of Venus. But these two diagrams * will show some of the circumstances of the very important phenomena, the times of internal contact at ingress and egress, the times when Venus is just completely on the Sun and just about to begin to go off. Great preparations were made for observing these times of ingress and egress, and the results would undoubtedly have been successful had it not been for the cruel way in which the geometrical sharpness of the phenomenon is ruined by the lighting up of the atmosphere of Venus; there was no instant when tangency was perceptible, and, to be frank, the Transit of Venus as a means of determining the distance of the Sun was a failure. The photographic and heliometer observations had for various reasons met with no better success than the observations of contacts; there was no consistency about the results.

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* [Not reproduced.—Eds.]

But just as the preparations for the Transits were beginning in 1867, Prof. Simon Newcomb had published an elaborate discussion of the Solar Parallax based upon several different methods. With some of these we are already familiar, and I will call attention to one only, the last, which we have not as yet discussed.

Components of Newcomb's Value.

	π.
Newcomb, Obs. of Mars, 1862	8″·855
Hall, ,, ,,	8 ·842
Hansen, Stone, and Newcomb, from Parallactic Inequality of Moon	8 ·838
Newcomb, Lunar Equation of the Earth	8 ·809
Powalky, Transit of Venus, 1769	8 ·832
Foucault's Velocity of Light and Struve's Aberration Const.	8 ·860
Weighted Mean	8 ·848

It is an effect of aberration that every star describes yearly in the sky an ellipse of which the semi-major axis is about 20″·5, and this number is called the constant of aberration. It is the ratio of the velocity of the Earth in its orbit to the velocity of light. When the constant is known and the velocity of light is known, the velocity of the Earth in its orbit is known; and since the time of describing that orbit is also known, the size of the orbit and the distance of the Earth from the Sun follow immediately.

In 1876 it appeared then that there was strong evidence against the value 8″·95, and without waiting for the results of the Transit of Venus expeditions, the *Nautical Almanac* adopted for the while the value 8″·848 found by Newcomb from this galaxy of results which looked so accordant; and that value was first used in the Almanac for 1882, the year of the second transit.

But meanwhile Sir David Gill, who had observed the transit of 1874 at Mauritius and had made up his mind very definitely that no good would come out of the transit of 1882, had borrowed Lord Lindsay's heliometer and established himself on the island of Ascension to observe with the heliometer the opposition in 1877 of the planet Mars. Every night the observing station in Mars Bay was carried some six or seven thousand miles by the rotation of the Earth and the planet thereby displaced among the stars by some 40″. The heliometer is by far the most refined instrument for the visual measurement of distance from star to star; the observations extended over months instead of hours; they could be pursued without any of the disquieting feelings that a temporary breakdown would ruin everything; and they were brought to a successful end in a parallax of 8″·78. But one doubt was cast upon the result. Was it possible that the red colour of Mars had influenced the measures systematically? It could not be denied that the effect of the dispersion of the air, which gave the planet a blue fringe above that might be lost in the blue sky, and a red fringe below that would be indistinguishable from the red planet itself, might have produced some effect; and Sir David Gill resolved to try again, utilizing this time three minor planets further away than Mars, with less parallax therefore but with discs so small that they were indistinguishable from stars.

In 1888 and 1889, five observatories, the Cape in the Southern hemisphere, and Yale, Göttingen, Leipzig, and the Radcliffe Observatory at Oxford in the Northern hemisphere, combined to observe the planets Victoria, Iris, and Sappho with the heliometer. The labour was immense. The observations proved to be so accurate that they demanded the use in a great part of the work of eight-figure logarithms. When, only a few years ago, the whole work was published in two enormous volumes of 'Annals of the Cape Observatory,' it might well have been thought that here was the last word of observation for many years. Yet we are now attacking the problem with more energy than ever!

About ten years ago the end of the century was in sight, and there was a general impression abroad that it was time to set one's house in order and to make a good start on the first of January, 1901. The directors of the four Nautical Almanacs (the British, French, German, and American) resolved to meet in Paris in 1896, and with the help of certain distinguished advisers to agree upon a uniform set of constants to be adopted in all the Almanacs from the year 1901. Among these constants was the Solar Parallax. We may summarize the information which was at the disposal of the conference thus:—

Solar Parallax from

Gill's Heliometer, Minor Planets	8″·802
Constant of Aberration of Light	8 ·799
Parallactic Inequality of Moon	8 ·794
Mass of Earth from motion of Node of Venus alone ..	8 ·762
,, ,, from secular var. of four inner planets ..	8 ·759

Gill's heliometer measures of minor planets gave 8″·802, and no other direct observational result could be compared with this; the Transits of Venus were discredited even though some of the final results had not, and have not even now, been published. The most recent determinations of the constant of the aberration of light gave 8″·799, the parallactic inequality of the Moon 8″·794. There were thus three powerful methods which converged upon a value close to 8″·80. But to set against them was a method which we have not yet noticed.

The perturbations in the motions of other planets produced by the Earth depend upon the mass of the Earth, and from them that mass can be determined. There is further a well-known relation between the mass of the Earth, the value of the gravitation constant, the length of the year, and the distance of the Sun, from which the latter may therefore be derived when we know the others. Prof. Newcomb had thus determined the parallax in two different ways, and had found two results agreeing closely among themselves, with mean 8″·76, but differing widely from the others. No explanation of this divergence could be found. But the evidence was 3 to 1 in favour of 8″·80, and 8″·80 was adopted in 1896 as the value to be used in all the Almanacs from the beginning of this century.

It might well have been thought that the question would have been allowed to rest there for a while. At the end of a century of labour four principal results had emerged, and there was a majority of 3 to 1 in favour of 8″·80. But there is a phenomenon, known in politics as the swing of the pendulum or the flowing tide, by whose operation a majority hardly won begins immediately to melt away. A like phenomenon appears to affect the Solar Parallax. We have seen how its adopted value has swung from 8″·57 to 8″·95, and back again to 8″·85 and 8″·80. Scarcely had the resolution of the Paris Conference been taken than the majority in favour of 8″·80 began to melt away. The beginning of the century had been chosen as an auspicious moment in which to make a change, without considering that there were at the end of the preceding century many investigations just then drawing to a close. The value of the Aberration Constant corresponding to 8″·80 is 20″·478. Almost every determination of that constant published since 1896—and there are many—had come out above 20″·50, many of these a long way above. Further investigation of the parallactic inequality of the Moon had not only altered the observed value of the inequality, but had modified the theoretical relation by which the parallax is deduced therefrom. The evidence for 8″·80 was giving way badly; and before the 1901 Almanac came into use, we had this revised table propounded by one of the chief instigators of the adoption of 8″·80. The majority was now 3 to 1 in favour of a value at least as low as 8″·77.

Revised Table.

Solar Parallax from	
Gill's Heliometer, Minor Planets	8″·802
Constant of Aberration of Light	8 ·77
Parallactic Inequality of Moon, probably	8 ·77
Mass of Earth from secular variations	8 ·759

[To be continued.]

The Total Solar Eclipse of 1905, *August* 30.

The state of tension amongst the various eclipse parties, and indeed amongst the public generally, is now over. At this early stage we can do no more than give a general idea of the success attending the expeditions so far as weather is concerned. Definite results will come after the development and discussion of the various photographs. It may be well to recall a few of the principal features. The central line of the eclipse ran through Arabia, across the Red Sea, and along the northern part of Africa, across the Mediterranean and Northern Spain. After crossing the Atlantic it crossed Labrador.

Prof. Turner and Mr. Bellamy, at Assouan, in Egypt, had a cloudless sky, somewhat hazy. The corona had prominent streamers in the sun-spot zones, those on the eastern side being the longer and less divergent, the longest, opposite the large sun-spot, being about two million miles. The hooked rays peculiar to the 1870 eclipse were visible at the northern pole, and many large prominences were seen.

Passing to Sfax, where the Astronomer Royal and Mr. Dyson are located, we find that close up to the eclipse the Sun was obscured by clouds, which fortunately cleared sufficiently to permit of successful observations. The appearance of the corona was that of pink flames tinged with magenta, and in the north-east and south-west direction extended to two solar diameters. Totality lasted 3 minutes 25 seconds, the temperature dropped 10 degrees, and many stars were seen. Sir William Christie anticipates good results from the photographs, which are on a four-inch scale.

We are extremely glad to record success at the next post. Prof. Todd, at Tripoli, had excellent weather, and managed with his automatic telescopic camera to obtain no less than 250 photographs. At Guelma, in Algeria, where M. Trépied, of the Observatory at Algiers, and MM. Stephan and Borelly, of Marseilles, are stationed, splendid weather was experienced, Mercury, Regulus, and Venus being seen as soon as the light began to fail. The solar corona was brilliant, not extensive, and fairly evenly distributed around the Sun's limb. Some fine prominences were seen, and the phenomenon of Bailly's beads is especially mentioned. The eclipse wind rose at beginning of totality, and moving shadow-stripes were seen along the ground. At Malta the weather was favourable. At Palma (Majorca) is a large concourse of astronomers, including Sir Norman Lockyer, Dr. Lockyer, Prof. Leahy of Sheffield, M. Gautier of Geneva, Drs. Black and Hunter of Edinburgh, Mr. and Mrs. Crommelin, Miss Everett, and Mr. and Mrs. Hollis as representing the *Observatory*. Mr. Hollis telegraphs that the corona was of the type observed by Brothers in 1870, with a fairly distributed corona, and hooked streamers at the poles. Clouds prevented perfectly satisfactory observations of the extent of the streamers, which probably stretched to two solar diameters. Prominences were seen on the north-east and south-west limbs. Mercury, Venus, and the more prominent stars of Leonis were seen.

Many other reports are to come in, but the above are ample to show that the 1905 eclipse has been a most successful one from an observational point of view, and the various expeditions are to be congratulated.

The stay-at-homes have not been so fortunate. Elaborate preparations were made at Greenwich, not only to take photographs, but to obtain observations for fixing the place of the Moon. The sky was overcast, and observations were quite hopeless. An energetic observer who ascended to the Golden Gallery of St. Paul's Cathedral just glimpsed the Sun; at the Marble Arch it was seen several seconds at a time. In the country the weather was more favourable, and one of our morning papers* bases a leading article on this fact, "Why Greenwich Observatory ought to be moved."

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The "False Dawn," or Zodiacal Light.

Gentlemen,—

The second stanza of the Rubáiyát of Omar Khayyám (*i. e.* Omar, the tent-maker), the Persian scientific poet who died in A.D. 1123, begins, as translated by Edward Fitzgerald (whose life by Mr. A. C. Benson has just been published):

> Dreaming when Dawn's left hand was in the sky,

or, as altered in the fourth edition,

> Before the phantom of false morning died.

Fitzgerald states, in a note, that the original word for "dawn's left hand" or "false morning" is *Subhi kázib*, and explains that this is a transient light on the horizon about an hour before the *Subhi sádik* or true dawn, a well-known phenomenon in the East. But he does not give (perhaps he was not acquainted with it) the name now recognized in science for this phenomenon. It is of course the zodiacal light, of which no western astronomer appears to have spoken before Kepler, and which acquired its name from Cassini† in 1683. Respecting this I wrote in the nineteenth volume of the *Observatory* (p. 274), and pointed out that the first to give what is undoubtedly a real description of it was Joshua Childrey, in his 'Britannica Baconica,' which was published in 1660.

The late Sir J. W. Redhouse, in the 'Journal of the Royal Asiatic Society,' speaks of the "false dawn" or "wolf's tail"

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* *Daily Mail*, 1905, August 31.

† His expression is "la lumière répanduë sur le zodiaque."

(as it was sometimes called) as being well known in the East. It may appear strange that they should have noticed the zodiacal light in the morning before sunrise, but not in the evening after sunset. Possibly this may be due to the eagerness with which Orientals would watch for the first appearance of the dawn of day (Ps. cxxx. 6).

Omar was one of those who were consulted for the reform of the Persian Calendar, and is said to have discovered that the accuracy which would be attained by arranging eight intercalary days in 33 years (equivalent to dropping a leap-year at the end of each period of 132 years) would be more precise than the Gregorian reckoning.

If it were made a rule to drop a leap-year at the end of each 128 years, this would be more accurate still, and, it seems to me, more convenient than our present system. It would amount to having 97 common years and 31 leap-years in each 128 years, which would therefore contain 46,751 days, so that each year would have on an average 365·2422 days, which is practically exact.

Recurring to the subject of the zodiacal light. It has been thought likely that in former ages (even historic) of the world's history the solar corona may have been more distinct than it is now, and occasionally perceptible even when there was no eclipse. The first, I believe, to suggest this was Dr. (now Sir William) Huggins in his paper on the "Corona of the Sun," which formed the Bakerian Lecture of the Royal Society in the year 1885, and on which you did me the honour of publishing a letter from me in the following year (*Observatory*, vol. ix. p. 128), where I pointed out that Grant was wrong in supposing that there was an eclipse on the occasion in question, A.D. 96. Now if the corona was formerly appreciably brighter than it is at present, the zodiacal light may not improbably have been so too, as there can be little doubt that the phenomena are connected. This consideration, then, confirms the view that the descriptions (which are quoted in my letter, vol. xix. p. 274) given, one by Diodorus Siculus of an appearance called by him *δοκίς πυρίνη*, and seen in B.C. 373, two years before the battle of Leuctra, and another, in the ecclesiastical history of Nicephorus, of a strange light seen for several months in the year A.D. 410, when Rome was taken by Alaric, may really have been manifestations of the zodiacal light. Otherwise the earliest reference to this interesting phenomenon is that contained in the poem of Omar Khayyám (as quoted in the beginning of this letter), which has only of late years become accessible to English readers by Fitzgerald's translation.

Yours faithfully,
W. T. Lynn.

Blackheath, 1905, July 3.

Dr. Galle and the Discovery of Neptune.

Gentlemen,—

I have not seen it mentioned in any English book that what led Le Verrier to make that communication of the result of his calculation of the place of a transuranian planet to Berlin was that a copy had recently been sent him of Galle's inaugural dissertation "Olai Roemeri triduum observationum astronomicarum," in which he gives the results of a reduction of the observations which were made by Römer at what he called his Tusculan Observatory, erected by him at Vridlösemagle, a few miles to the west of Copenhagen, from October 20 to 23, 1706, the records of which were all that remained after the great fire at Copenhagen in 1728. The observations, with a description of the instruments, are given in Peter Horrebow's 'Basis Astronomiæ,' and the reason apparently why the expression Triduum is used, although there are observations on each of the *four* days October 20 to 23, is that they extend "ab hora quarta pomeridiana diei 20 Octobris ad horam pomeridianam sextam die 23 Octobris." They include meridian observations of the Sun, Moon, planets, and stars; also observations of a large partial eclipse of the Moon which took place on October 21.

The Tusculan Observatory was erected in 1704; Römer observed there continuously till near the time of his death in September 1710. As the 'Penny Cyclopædia' is still a useful book of reference, I may note that it erroneously gives the date of the last event in 1702. After the fire in 1728 the old observatory at Copenhagen was rebuilt, but the present one, much better adapted to the needs of modern astronomy, was not established until 1861.

In conclusion, let me express the hope that Dr. Galle, who bore so worthy a part in the discovery of Neptune as actual observer, may live to see the "diamond jubilee" of that event in September next year.

Yours faithfully,
W. T. LYNN.

Blackheath, 1905, Aug. 1.

D_3 *as a Dark Line in the Solar Spectrum.*

GENTLEMEN,—

Since April 1904 it has been part of the routine work of this Observatory to examine the regions surrounding spots for D_3 as a dark line, and I hope very shortly to publish a list of 77 dates on which the search has been successful. Most of the observations have been made by two of my assistants, but I have checked the work personally several times, and have no doubt as to the reality of the appearance. As a general rule, spots showing much disturbance in the hydrogen lines also show D_3 as a dark line, but there have been cases in which C has been greatly disturbed, while no trace could be found of dark D_3.

Yours faithfully,
C. MICHIE SMITH.

The Observatory, Kodaikanal,
1905, July 25.

NOTES.

Minor Planet Notes.—The following new planets have been discovered:—

Planet.	Date.	Discoverer.	Place.	Magnitude.
QS ..	1905 July 27	Götz.	Heidelberg.	12·3
QT ..	Aug. 1	Palisa.	Vienna.	13·0
QU ..	July 30	„	„	12·5
QV ..	July 30	Wolf.	Heidelberg.	12·8
QW ..	1904 Apr. 4	Frost.	Arequipa.	14 (?)

A planet named QR proved not to be new, but identical with 216 Cleopatra; the ephemeris of this planet in the 'Berliner Jahrbuch' had been erroneously computed.

Dr. Palisa is to be congratulated on a renewal of his visual discoveries, the first after an interval of several years.

QW was at first supposed to be 475 Ocllo, but this proves not to be the case. Ocllo has, however, been recovered in the present year at Arequipa.

QW was subsequently found on a Heidelberg plate taken 1904 March 20.

The following planets have been named:—489 Comacina, 504 Cora, 505 Cava, 511 Davida. The second and third names are taken from Peruvian Mythology. A. C. D. C.

The Sixth and Seventh Satellites of Jupiter.—Both of these satellites have been recently photographed by Mr. Albrecht at the Lick Observatory, the positions being:—

G.M.T.	Satellite.	Position-angle.	Distance.
1905 July 25·95	VI.	55°·0	25′·1
26·97	„	52 ·7	24 ·3
27·93	„	50 ·7	23 ·6
Aug. 7·96	VII.	289 ·7	54 ·6

From the new observations of VI. I have deduced approximate elements as follows:—Period 253 days, eccentricity 0·16, N. Pole of orbit plane R.A. 90°, N. Dec. 87°, longitude of perijove 269°.

Dr. Ross has published an orbit of VII.: Period 265 days, eccentricity 0·025, N. pole of orbit R.A. 191°, N. Dec. 64°, longitude of perijove 337°, motion direct. His predicted place for August 8 is position-angle 294°, distance 24′. The error in the distance is so great that evidently his elements are not very near the truth. A. C. D. C.

The Constant of Aberration.—Prof. Doolittle, of the Flower Observatory, Pennsylvania, who has devoted himself a great deal to determination of this constant, has published (*Astronomical Journal*, No. 571) a value 20″·54 as the result of more than 15,000 determinations. Prof. Doolittle remarks that no reasonable changes in weighting will alter this result more than 0″·01. It need scarcely be recalled that Struve's value 20″·445 was used for many years in the *Nautical Almanac*, that in 1896 the Paris Conference adopted 20″·47, and that in 1903 Dr. Chandler stated, as the result of a very complete investigation, that the real value is likely to be found near or slightly above 20″·52.

THE following is a summary of work done by various parties during the Solar Eclipse:—

Sfax.........	Sir W. Christie, Mr. Dyson, Mr. Davidson, M. Bigourdan.	Cloudy at first, clear during totality. Photographs on 4″ and 1½″ scales for inner corona and extension were obtained. Spectrograms successful by Mr. Dyson. Temperature fell; humidity increased.
Assouan ...	Prof. Turner, Mr. Bellamy, Prof. Hussey.	Successful polariscopic observations; large-scale photographs of corona; spectrographs. Totality 2^m 24^s. Principal streamers in S.E. Eight plates exposed for intra-mercurial planet.
Guelma ...	Mr. Newall, M. Trépied.	Splendid weather; corona brilliant and uniformly distributed. Fine prominences. Baily's beads. Spectroscopic and polariscopic observations successful.
Palma......	Sir N. Lockyer, Dr. Lockyer, Mr. Crommelin, Dr. Hunter.	Not complete success. Clouds. Chromosphere and corona with prismatic camera. Small-scale photographs of corona. Type 1870 corona.
Tripoli......	Prof. Todd, Prof. Millesordei, M. Liberd.	Totality 3^m 2^s. American, Italian, and French parties successful. Shadow-bands five. Baily's beads not seen. No long streamers. 250 photographs.
Burgos......	Mr. J. Evershed.	Photographs before totality a failure owing to clouds. Photographs of chromosphere and corona with prismatic reflector. It is hoped that work during totality will be good.
Oropesa ...	Prof. Callendar, Prof. Fowler, Mr. Shackleton.	Totality obscured by clouds. First and last contacts observed. Good records of radiation and temperature.

A very good idea of the work done all over England will be found in the *Yorkshire Post* for Thursday, August 31.

POPULAR METEOROLOGY.—In order to advance the general knowledge of Meteorology and to promote an intelligent popular interest in the science, the Meteorological Council have appointed a Lecturer who is prepared to deliver lectures on meteorological subjects. Societies and institutions wishing such lectures will be expected to pay a moderate fee and to defray travelling expenses. The Council will also arrange small meteorological exhibitions at scientific gatherings, institutions, or schools at a small cost.

From an Oxford Note-Book.

Cairo*, 1905, Aug. 1–7.

Three times within the last eight years has it fallen to my lot to start *viâ* Marseilles for the observation of a Total Solar Eclipse —towards India in 1898, towards Algiers in 1900, and now towards Egypt in 1905. (A fourth journey might also have commenced in the same way if I had been able to go to Sumatra in 1901: but against this possibility the Fates decided adversely.) Among journeys which aim at complete change of scene and climate within 24 hours that from London to Marseilles is capable of ranking high; but the summer is scarcely the time to bring out this particular feature. It was summer in London when I started, and not more than summer in Marseilles on arrival. Now in the winter 1897-8, and in the spring of 1900, the journey brought as its reward a gain of at least half-a-season. This time we gained only heat and dirt.

But on the good P. & O. Steamer 'Himalaya' (was it not in another 'Himalaya' that they started out to observe the eclipse of 1860?) there is a room full of beautiful appliances for squirting water at you in all possible ways; and under this efficient treatment the dirt of the railway journey was soon removed. We put to sea in the finest and calmest weather on Friday morning, July 28, and those who had come through the Bay of Biscay told us that it was "the roughest day they had had since leaving London." The same weather accompanied us to Port Said. We had a pleasant and interesting company on board; and as an illustration of the rapid changes which have taken place recently in the administration of North Africa, I may remark that my neighbour at dinner was returning to his duties as Acting Governor at Fashoda, with a province as big as France and Spain put together; and was ready to regard the circumstance as commonplace.

Now the rest of the doings of that voyage, and the runs that we made at cricket, and how an astronomer bowled four wickets in five balls, and the points that were won and lost at bridge, are they not written on the score-sheets which were thrown or blown into the sea?

So we arrived at Port Said. And there met us Captain Lyons, Director-General of the Survey of Egypt; and he spoke unto the

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* The MS. of these Notes was lost in the post. What could be remembered has been written down three months afterwards (see p. 476).

Arabs language which they did understand, so that there was no trouble, not even the slightest, neither concerning our tickets, nor our baggage, nor our comfort, nor our departure for Cairo, nor our arrival, nor anything else.

AND in Cairo it is necessary to wait a few days for the ship containing Mr. Bellamy and the instruments and some dynamite, all of which started some weeks ago, and is apparently making a careful and leisurely passage. But the necessity for lingering here is a very agreeable one, save only for the sand-fly. The warmth is not unpleasant, and the mosquito is an old acquaintance, who, moreover, knows and respects a mosquito-curtain when he sees it. But the sand-fly seems to have no traditions of the kind, and I would gladly have foregone an introduction to him. But I am told that he is received everywhere, even in the highest circles, so that it is impossible to avoid meeting him. I can only hope that our intimacy may not develop much further.

THERE are many things to see in and near Cairo. There is, for instance, the Survey Office, where an army of workers is rapidly evolving order from chaos, and producing an intelligible map of the country. Some of the difficulties of doing this might not suggest themselves at once to the lay mind. In the ordinary course a Survey Officer expects to deal with more or less permanent features: he surveys a field in the full expectation that its boundaries will remain where they are. But in Egypt boundaries are annually marked out in mud and annually deleted, and the element of permanence is supplied from tradition aided by a few stones. Hence it is necessary to study these traditions in order to reconcile them, if possible, with the Survey. Sometimes they are a little confused, especially in the orientations; for the general tendency of the river Nile to run north has led to the adoption of "down-stream" as a sufficiently accurate equivalent for "north" in many cases where the two directions are widely at variance, and it is readily seen what fine possibilities there are here for entanglement. Then the small size and consequent great number of the territories is an obvious source of trouble: they have been subdivided in the transmission from father to son until they are almost microscopic on any self-respecting map. From which it will be clear that the task set before Captain Lyons and his staff is in many ways exceptional.

THAT they will be equal to the situation there can be no doubt. The most casual inspection of the Survey Office carries conviction of its efficiency. There are armies of computers and draughtsmen collected into big rooms where they can be superintended with the maximum of economy: they are provided with the most modern appliances for computing and drawing—for instance, with machines for plotting points from rectangular coordinates, and with Brunsviga arithmometers; and they start work at sunrise when the climate is most conducive to work. There is plenty of elbow-room everywhere without waste. We found Mr. Kearney (who photographed the eclipse of 1893 as Sergeant Kearney, but has since left the service) at work in a suite of spacious dark-rooms such as probably has not its equal anywhere; and the consequent convenience of manipulation is undoubtedly represented by increased general excellence in the output. Finally, it is worthy of note that the card-catalogue system is in use throughout the office with a completeness and efficiency which I have not seen surpassed even in the United States.

HAVE you ever seen a Brunsviga figured with Arabic figures? I always thought our ordinary numerals were Arabic, but that comes of not reading the 'Encyclopædia Britannica.' They are really Indian, and the Arabs use figures differing from them essentially. They use our zero symbol to mean five, and a decimal point has to do duty for zero. Our figure seven is six with them, and they use a V for seven; and so on. Captain Lyons found it was much easier to have machines figured in this way than to teach his Arabs our numerals; and indeed their system is actually much quicker to write than ours, the figures being simpler on the whole.

AND this is the story of the Dear Old Lady and the Arithmometer:—She was shown one of these instruments and its use explained, to her obvious bewilderment. But the marvellous properties of mechanism gradually dawned upon her: her face brightened and she said "Then there *may* be something in a Chinese praying-wheel after all!"

THE Survey Office is one of the more modern buildings in Egypt, having been built at Giza in 1900 (A.D.). There are in the same neighbourhood some older buildings, pyramidal in shape, which have sometimes attracted attention. We went there by electric car and donkey, the first method of progression being preferable. We also visited the new observatory at Helwan, which bids fair to rival the triple institute at Potsdam in completeness. There is a complete system of magnetic records; there is a fine battery of

Callendar recorders for all sorts of meteorological observations; a building has just been completed for comparing standards for geodetic work; and, finally, the astronomical equipment is growing satisfactorily. A fine reflecting telescope is being presented to the Observatory by Mr. J. H. Reynolds, of Birmingham, the mirror of 30 in. aperture having been made by the late Dr. Common (a companion to the one at Greenwich) and the mounting having been designed by Mr. Reynolds himself and constructed under his direction. He is to bring this fine instrument out with him in a fortnight's time, and hopes to see it set up at Helwan after he has observed the eclipse and before he leaves Egypt. Indeed he is so enthusiastic about getting a telescope planted in Egypt worthy of its beautiful climate that even the eclipse takes a secondary place in his thoughts alongside the erection of this 30-in. reflector.

The reflector will be mounted in the dome which formerly contained only an 8-in. refractor, but was wisely built large enough for a bigger instrument. In the interval between the dismounting of one instrument and the erection of the other, Mr. B. H. Wade, who is in charge of the observatory, is making some interesting experiments with a modest apparatus, on getting photographs of Moon and stars on the same plate, for determination of the Moon's place among the stars. In this beautiful climate the Moon can be observed night after night, all round the lunation.

The old observatory at Abbassia, where Professor Pritchard made his photometric observations for comparison with those made at Oxford, is now almost abandoned in favour of that at Helwan. Indeed it has been occupied as a recruiting building. But there is still, for the sake of continuity, a set of meteorological instruments, which are regularly read; and the observer who reads them also fires the time-gun.

And this is the story of the Man-who-used-to-fire-the-Gun and the Watchmaker:—In the old days, before the modern regime, enquiry was made of the Man how he knew when to fire the Gun, and he replied that he knew it from his watch. "But," went on his questioner, "how do you know that your watch is right?" "Because," he answered, "I send it once a week to the good Watchmaker and he puts it right for me." Filled with a still unsatisfied curiosity, the questioner then left the Man and proceeded to the dwelling of the Watchmaker. "I learn," said he, "that you are able to put right the Man's watch when he sends it to you weekly?" "Indeed, Sir! and you have been rightly informed," was the reply. "Then may I ask," pursued the remorseless enquirer, "how do you know the right time by which to correct the watch?" "Indeed you may, Sir! and no question is easier to answer. I get the correct time from the Gun."

WE have also visited the Zoological Gardens in Cairo. Many strange beasts are in these Gardens and the youthful Arab gazes at them with a mild surprise: but there is one which simply fascinates him—so that he follows it about the Gardens in attentive crowds; it is a camel with two humps. I suppose it is as strange to him as a man with two noses, and stranger far than the newly discovered fossil animal with two curious horns called, I think, *Arsinoitherium Zittelli*, a fine head of which is preserved in the Museum attached to the Survey Department. I had almost forgotten to mention this Museum; but the fact is, the Survey Department has so many interesting things in it one is bound to forget some of them. That this curious animal was not forgotten is due chiefly to the interesting story of its discovery. A naturalist wishing to find fossils or bones enquired of the Survey Office for a good hunting-ground, and one of the staff (Mr. Beadnell, if I remember rightly) recollected that he had noticed some bones lying about in a certain place when at work on the triangulation. He conducted the naturalist to the spot and various satisfactory things were found. The incident might have closed there, but for the enthusiasm of Mr. Beadnell, which carried him back to the spot afterwards on his own account, and then it was that he found two or three specimens of this new fossil beast. Part of the find was duly sent to the Natural History Museum in London, but a portion was very reasonably retained in Cairo.

THERE is a phenomenon visible after sunset here which I have watched with some interest: viz. the after-glow. Is it real or an illusion due to contrast? If real, the phenomenon should be reversible, and there should be a preliminary glow before sunrise, which is probably the False Dawn, which I was asked to explain, some months ago, by an Egyptian gentleman resident in Oxford. My questioner was desirous of observing a Mahommedan fast-day according to a strict rule which enjoins that it shall begin at the False Dawn. He described the phenomenon as he had seen it in Egypt and asked for the G.M.T. of its occurrence in Oxford. My first thought was that it must be the rising of the Zodiacal Light *, but such evidence as I could collect did not support this view. I have searched some literature without much success: even that great work 'The Dawn of Astronomy' seems to have been written without any suspicion that there could be a "False Dawn." Perhaps the simplest plan for investigating the matter would be to awake early some morning and look at it. I must discuss this enterprise with my friend the sand-fly.

NEWS has come of Mr. Bellamy and the instruments: they will be here in Cairo to-morrow (Saturday, August 5), and on Monday evening we all start on the 24 hours' journey to Aswan. There is, we learn, a large hotel open even now. In the winter there are three such, and they barely suffice for the crowds of visitors; but during the eight months' summer a visitor is a rarity. Nevertheless for the last two summers one hotel has been kept open, and we shall reap the benefit of the innovation. Captain Lyons is coming to see us comfortably established and will leave Mr. Keeling, one of his Survey officers, in charge of us, to talk Arabic to the natives and look after our wants generally. But, from the experience we have had so far, it seems improbable that we shall have many wants.

THE eclipse has already begun to attract attention in the papers. We read in the *Egyptian Gazette* that "at Cairo the area of the eclipsed portion of the Sun will be ·9047 m." The idea of a mere number without any dimensions was apparently repellent to the printer, and so he has made it "metres," *faute de mieux*. "At Alexandria the eclipsed area will measure ·88 m. . . . . At Port Said the eclipsed area will measure ·87 m.," and so on.

WE hear, too, rumours that the British expedition which was to take up its station at Philippeville for the eclipse has been refused permission "on the pretext of the exigencies of national defence"; and the following paragraph has made some of our eyes open:—

> It is estimated at Philippeville that the General's prohibition occasions a commercial loss of 200,000 francs, as extensive orders had been conditionally placed for the construction of observation towers for use with the various instruments.

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* *Note added* 1905, *November* 18.—I see that Mr. W. T. Lynn takes this view in his interesting letter on p. 356 of this volume, and for all I know he may be right. The phenomenon was observed at Aswan: see p. 398.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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Meeting of the British Association in South Africa, 1905.— Address by the President, Prof. G. H. Darwin, M.A., F.R.S.

Cosmical Evolution.

[Continued from p. 343.]

We have seen that, in order to explain the genesis of planets according to Laplace's theory, the rings must be ill-balanced or even broken. If the ring were so far from being complete as only to cover a small segment of the whole circumference, the true features of the occurrences in the births of planets and satellites might be better represented by conceiving the detached portion of matter to have been more or less globular from the first, rather than ring-shaped. Now this idea introduces us to a group of researches whereby mathematicians have sought to explain the birth of planets and satellites in a way which might appear, at first sight, to be fundamentally different from that of Laplace.

The solution of the problem of evolution involves the search for those persistent or stable forms which biologists would call species. The species of which I am now going to speak may be grouped in a family, which comprises all those various forms which a mass of rotating liquid is capable of assuming under the conjoint influences of gravitation and rotation. If the Earth were formed throughout of a liquid of the same density, it would be one of the species of this family; and indeed these researches date back to the time of Newton, who was the first to explain the figures of planets.

The ideal liquid planets we are to consider must be regarded as working models of actuality, and inasmuch as the liquid is supposed to be incompressible, the conditions depart somewhat widely from those of reality. Hence, when the problem has been solved, much uncertainty remains as to the extent to which our conclusions will be applicable to actual celestial bodies.

We begin, then, with a rotating liquid planet like the Earth, which is the first stable species of our family. We next impart in imagination more rotation to this planet, and find by mathematical calculation that its power of resistance to any sort of disturbance is less than it was. In other words, its stability declines with increasing rotation, and at length we reach a stage at which the stability just vanishes. At this point the shape is a transitional one, for it is the beginning of a new species with different characteristics from the first, and with a very feeble degree of stability or power of persistence. As a still further amount of rotation is imparted, the stability of the new species increases to a maximum and then declines until a new transitional shape is reached and a new species comes into existence. In this way we pass from species to species with an ever-increasing amount of rotation.

The first or planetary species has a circular equator like the Earth; the second species has an oval equator, so that it is something like an egg spinning on its side on a table; in the third species we find that one of the two ends of the egg begins to swell, and that the swelling gradually becomes a well-marked protrusion or filament. Finally the filamentous protrusion becomes bulbous at its end, and is only joined to the main mass of liquid by a gradually thinning neck. The neck at length breaks, and we are left with two separated masses which may be called planet and satellite. It is fair to state that the actual rupture into two bodies is to some extent speculative, since mathematicians have hitherto failed to follow the whole process to the end.

In this ideal problem the successive transmutations of species are brought about by gradual additions to the amount of rotation with which the mass of liquid is endowed. It might seem as if this continuous addition to the amount of rotation were purely arbitrary and could have no counterpart in nature. But real bodies cool and contract in cooling, and, since the scale of magnitude on which our planet is built is immaterial, contraction will produce exactly the same effect on shape as augmented rotation. I must ask you, then, to believe that the effects of an apparently arbitrary increase of rotation may be produced by cooling.

The figures which I succeeded in drawing, by means of rigorous calculation, of the later stages of this course of evolution, are so curious as to remind one of some such phenomenon as the protrusion of a filament of protoplasm from a mass of living matter, and I suggest that we may see in this almost life-like process the counterpart of at least one form of the birth of double stars, planets, and satellites.

As I have already said, Newton determined the first of these figures; Jacobi found the second, and Poincaré indicated the existence of the third, in a paper which is universally regarded as one of the masterpieces of applied mathematics; finally I myself succeeded in determining the exact form of Poincaré's figure, and in proving that it is a true stable shape.

My Cambridge colleague Jeans has also made an interesting contribution to the subject by discussing a closely analogous problem, and he has besides attacked the far more difficult case where the rotating fluid is a compressible gas. In this case also he finds a family of types, but the conception of compressibility introduced a new set of considerations in the transitions from species to species. The problem is, however, of such difficulty that he had to rest content with results which were rather qualitative than strictly quantitative.

This group of investigations brings before us the process of the birth of satellites in a more convincing form than was possible by means of the general considerations adduced by Laplace. It cannot be doubted that the supposed Laplacian sequence of events possesses a considerable element of truth, yet these latter schemes of transformation can be followed in closer detail. It seems, then, probable that both processes furnish us with crude models of reality, and that in some cases the first and in others the second is the better representative.

The Moon's mass is one-eightieth of that of the Earth, whereas the mass of Titan, the largest satellite in the solar system, is $\frac{1}{4600}$ of that of Saturn. On the ground of this great difference between the relative magnitudes of all other satellites and of the Moon, it is not unreasonable to suppose that the mode of separation of the Moon from the Earth may also have been widely different. The theory of which I shall have next to speak claims to trace the gradual departure of the Moon from an original position not far removed from the present surface of the Earth. If this view is correct, we may suppose that the detachment of the Moon from the Earth occurred as a single portion of matter, and not as a concentration of a Laplacian ring.

If a planet is covered with oceans of water and air, or if it is formed of plastic molten rock, tidal oscillations must be generated in its mobile parts by the attractions of its satellites and of the Sun. Such movements must be subject to frictional resistance, and the planet's rotation will be slowly retarded by tidal friction in much the same way that a fly-wheel is gradually stopped by any external cause of friction. Since action and reaction are equal and opposite, the action of the satellites on the planet, which causes the tidal friction of which I speak, must correspond to a reaction of the planet on the motion of the satellites.

At any moment of time we may regard the system composed of the rotating planet with its attendant satellite as a stable species of motion, but the friction of the tides introduces forces which produce a continuous, although slow, transformation in the configuration. It is, then, clearly of interest to trace backwards in time the changes produced by such a continuously acting cause, and to determine the initial condition from which the system of planet and satellite must have been slowly degrading. We may also look forward, and discover whither the transformation tends.

Let us consider, then, the motion of the Earth and Moon revolving in company round the Sun, on the supposition that the friction of the tides in the Earth is the only effective cause of change. We are, in fact, to discuss a working model of the system, analogous to those of which I have so often spoken before.

This is not the time to attempt a complete exposition of the manner in which tidal friction gives rise to the action and reaction between planet and satellite, nor shall I discuss in detail the effects of various kinds which are produced by this cause. It must suffice to set forth the results in their main outlines, and, as in connection with the topic of evolution retrospect is perhaps of greater interest than prophecy, I shall begin with the consideration of the past.

At the present time the Moon, moving at a distance of 240,000 miles from the Earth, completes her circuit in twenty-seven days. Since a day is the time of one rotation of the Earth on its axis, the angular motion of the Earth is twenty-seven times as rapid as that of the Moon.

Tidal friction acts as a break on the Earth, and therefore we look back in retrospect to times when the day was successively twenty-three, twenty-two, twenty-one of our present hours in length, and so on backward to still shorter days. But during all this time the reaction on the Moon was at work, and it appears that its effect must have been such that the Moon also revolved round the Earth in a shorter period than it does now; thus the month also was shorter in absolute time than it now is. These conclusions are absolutely certain, although the effects on the motions of the Earth and of the Moon are so gradual that they can only doubtfully be detected by the most refined astronomical measurements.

We take the "day," regarding it as a period of variable length, to mean the time occupied by a single rotation of the Earth on its axis; and the "month," likewise variable in absolute length, to mean the time occupied by the Moon in a single revolution round the Earth. Then, although there are now twenty-seven days in a month, and although both day and month were shorter in the past, yet there is, so far, nothing to tell us whether there were more or less days in the month in the past. For if the day is now being prolonged more rapidly than the month, the number of days in the month was greater in the past than it now is; and if the converse were true, the number of days in the month was less.

Now it appears from mathematical calculation that the day must now be suffering a greater degree of prolongation than the month, and accordingly in retrospect we look back to a time when there were more days in the month than at present. That number was once twenty-nine, in place of the present twenty-seven; but the epoch of twenty-nine days in the month is a sort of crisis in the history of Moon and Earth, for yet earlier the day was shortening less rapidly than the month. Hence, earlier than the time when there were twenty-nine days in the month, there was a time when there was a reversion to the present smaller number of days.

We thus arrive at the curious conclusion that there is a certain number of days to the month, namely twenty-nine, which can never have been exceeded, and we find that this crisis was passed through by the Earth and Moon recently; but, of course, a recent event in such a long history may be one which happened some millions of years ago.

Continuing our retrospect beyond this crisis, both day and month are found continuously shortening, and the number of days in the month continues to fall. No change in conditions which we need pause to consider now supervenes, and we may ask at once, what is the initial stage to which the gradual transformation points? I say, then, that on following the argument to its end the system may be traced back to a time when the day and month were identical in length, and were both only about four or five of our present hours. The identity of day and month means that the Moon was always opposite to the same side of the Earth; thus at the beginning the Earth always presented the same face to the Moon, just as the Moon now always shows the same face to us. Moreover, when the month was only some four or five of our present hours in length the Moon must have been only a few thousand miles from the Earth's surface—a great contrast with the present distance of 240,000 miles.

It might well be argued from this conclusion alone that the Moon separated from the Earth more or less as a single portion of matter at a time immediately antecedent to the initial stage to which she has been traced. But there exists a yet more weighty argument favourable to this view, for it appears that the initial stage is one in which the stability of the species of motion is tottering, so that the system presents the characteristic of a transitional form, which we have seen to denote a change of type or species in a previous case.

In discussing the transformation of a liquid planet we saw the tendency of the single mass to divide into two portions, although we failed to extend the rigorous argument back to the actual moment of separation; and now we seem to reach a similar crisis from the opposite end, when in retrospect we trace back the system to two masses of unequal size in close proximity with one another. The argument almost carries conviction with it, but I have necessarily been compelled to pass over various doubtful points.

Time is wanting to consider other subjects worthy of notice which arise out of this problem, yet I wish to point out that the

Earth's axis must once have been less tilted over with reference to the Sun than it is now, so that the obliquity of the ecliptic receives at least a partial explanation. Again, the inclination of the Moon's orbit may be in great measure explained; and, lastly, the Moon must once have moved in a nearly circular path. The fact that tidal friction is competent to explain the eccentricity of an orbit has been applied in a manner to which I shall have occasion to return hereafter.

In my paper on this subject I summed up the discussion in the following words, which I still see no reason to retract:—

"The argument reposes on the imperfect rigidity of solids, and on the internal friction of semi-solids and fluids; these are *veræ causæ*. Thus changes of the kind here discussed must be going on, and must have gone on in the past. And for this history of the Earth and Moon to be true throughout it is only necessary to postulate a sufficient lapse of time, and that there is not enough matter diffused through space materially to resist the motions of the Moon and Earth in perhaps several hundred million years.

"It hardly seems too much to say that granting these two postulates and the existence of a primeval planet, such as that above described, then a system would necessarily be developed which would bear a strong resemblance to our own.

"A theory, reposing on *veræ causæ*, which brings into quantitative correlation the lengths of the present day and month, the obliquity of the ecliptic, and the inclination and eccentricity of the lunar orbit, must, I think, have strong claims to acceptance" *.

We have pursued the changes into the past, and I will refer but shortly to the future. The day and month are both now lengthening, but the day changes more quickly than the month. Thus the two periods tend again to become equal to one another, and it appears that when that goal is reached both day and month will be as long as fifty-five of our present days. The Earth will then always show the same face to the Moon, just as it did in the remotest past. But there is a great contrast between the ultimate and initial conditions, for the ultimate stage, with day and month both equal to fifty-five of our present days, is one of great stability in contradistinction to the vanishing stability which we found in the initial stage.

Since the relationship between the Moon and Earth is a mutual one, the Earth may be regarded as a satellite of the Moon, and if the Moon rotated rapidly on her axis, as was probably once the case, the Earth must at that time have produced tides in the Moon. The mass of the Moon is relatively small, and the tides produced by the Earth would be large: accordingly the Moon would pass through the several stages of her history much more rapidly than the Earth. Hence it is that the Moon has already advanced to that condition which we foresee as the future fate of the Earth, and now always shows to us the same face.

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* Phil. Trans. pt. ii. 1880, p. 883.

If the Earth and Moon were the only bodies in existence, this ultimate stage, when the day and month were again identical in length, would be one of absolute stability, and therefore eternal; but the presence of the Sun introduces a cause for yet further changes. I do not, however, propose to pursue the history to this yet remoter futurity, because our system must contain other seeds of decay which will probably bear fruit before these further transformations could take effect.

If, as has been argued, tidal friction has played so important a part in the history of the Earth and Moon, it might be expected that the like should be true of the other planets and satellites, and of the planets themselves in their relationship to the Sun. But numerical examination of the several cases proves conclusively that this cannot have been the case. The relationship of the Moon to the Earth is in fact quite exceptional in the solar system, and we have still to rely on such theories as that of Laplace for the explanation of the main outlines of the solar system.

I have as yet only barely mentioned the time occupied by the sequence of events sketched out in the various schemes of cosmogony, and the question of cosmical time is a thorny and controversial one.

Our ideas are absolutely blank as to the time requisite for the evolution according to Laplace's nebular hypothesis. And again, if we adopt the meteoritic theory, no estimate can be formed of the time required even for an ideal Sun, with its attendant planet Jove, to sweep up the wanderers in space. We do know, indeed, that there is a continuous gradation from stable to unstable orbits, so that some meteoric stones may make thousands or millions of revolutions before meeting their fate by collision. Accordingly, not only would a complete absorption of all the wanderers occupy an infinite time, but also the amount of the refuse of the solar system still remaining scattered in planetary space is unknown. And, indeed, it is certain that the process of clearance is still going on, for the Earth is constantly meeting meteoritic stones, which, penetrating the atmosphere, become luminous through the effects of the frictional resistance with which they meet.

All we can assert of such theories is that they demand enormous intervals of time as estimated in years.

The theory of tidal friction stands alone amongst these evolutionary speculations in that we can establish an exact but merely relative time-scale for every stage of the process. It is true that the value in years of the unit of time remains unknown, and it may be conjectured that the unit has varied to some extent as the physical condition of the Earth has gradually changed.

It is, however, possible to determine a period in years which must be shorter than that in which the whole history is comprised. If at every moment since the birth of the Moon tidal friction had always been at work in such a way as to produce the greatest possible effect, then we should find that sixty million years would be consumed in this portion of evolutionary history. The true period must be much greater, and it does not seem extravagant to suppose that 500 to 1000 million years may have elapsed since the birth of the Moon.

Such an estimate would not seem extravagant to geologists who have, in various ways, made exceedingly rough determinations of geological periods. One such determination is derived from measures of the thickness of deposited strata, and the rate of the denudation of continents by rain and rivers. I will not attempt to make any precise statement on this head, but I imagine that the sort of unit with which the geologist deals is 100 million years, and that he would not consider any estimate involving from one to twenty of such units as unreasonable.

Mellard Reade has attempted to determine geological time by certain arguments as to the rate of denudation of limestone rocks, and arrives at the conclusion that geological history is comprised in something less than 600 million years *. The uncertainty of this estimate is wide, and I imagine that geologists in general would not lay much stress on it.

Joly has employed a somewhat similar, but probably less risky, method of determination †. When the Earth was still hot, all the water of the globe must have existed in the form of steam, and when the surface cooled that steam must have condensed as fresh water. Rain then washed the continents and carried down detritus and soluble matter to the seas. Common salt is the most widely diffused of all such soluble matter, and its transit to the sea is an irreversible process, because the evaporation of the sea only carries back to the land fresh water in the form of rain. It seems certain, then, that the saltness of the sea is due to the washing of the land throughout geological time.

Rough estimates may be formed of the amount of river water which reaches the sea in a year, and the measured saltness of rivers furnishes a knowledge of the amount of salt which is thus carried to the sea. A closer estimate may be formed of the total amount of salt in the sea. On dividing the total amount of salt by the annual transport Joly arrives at the quotient of about 100 millions, and thence concludes that geological history has occupied 100 million years. I will not pause to consider the several doubts and difficulties which arise in the working out of this theory. The uncertainties involved must clearly be considerable, yet it seems the best of all the purely geological arguments whence we derive numerical estimates of geological time. On the whole I should say that pure geology points to some period intermediate between 50 and 1000 millions of years, but the upper

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* 'Chemical Denudation in relation to Geological Time,' Bogue, London, 1879; or 'Proc. Roy. Soc.,' January 23, 1879.

† "An Estimate of the Geological Age of the Earth," Trans. Roy. Dublin Soc. vol. vii. series iii. 1902, pp. 23–66.

limit is more doubtful than the lower. Thus far we do not find anything which renders the tidal theory of evolution untenable.

But the physicists have formed estimates in other ways which, until recently, seemed to demand in the most imperative manner a far lower scale of time. According to all theories of cosmogony, the Sun is a star which became heated in the process of its condensation from a condition of wide dispersion. When a meteoric stone falls into the Sun the arrest of its previous motion gives rise to heat, just as the blow of a horse's shoe on a stone makes a spark. The fall of countless meteoric stones, or the condensation of a rarefied gas, was supposed to be the sole cause of the Sun's high temperature.

Since the mass of the Sun is known, the total amount of the heat generated in it, in whatever mode it was formed, can be estimated with a considerable amount of precision. The heat received at the Earth from the Sun can also be measured with some accuracy, and hence it is a mere matter of calculation to determine how much heat the Sun sends out in a year. The total heat which can have been generated in the Sun divided by the annual output gives a quotient of about 20 millions. Hence it seemed to be imperatively necessary that the whole history of the solar system should be comprised within some 20 millions of years.

This argument, which is due to Helmholtz, appeared to be absolutely crushing, and for the last forty years the physicists have been accustomed to tell the geologists that they must moderate their claims. But for myself I have always believed that the geologists were more nearly correct than the physicists, notwithstanding the fact that appearances were so strongly against them.

And now, at length, relief has come to the strained relations between the two parties, for the recent marvellous discoveries in physics show that concentration of matter is not the only source from which the Sun may draw its heat.

Radium is a substance which is perhaps millions of times more powerful than dynamite. Thus it is estimated that an ounce of radium would contain enough power to raise 10,000 tons a mile above the Earth's surface. Another way of stating the same estimate is this: the energy needed to tow a ship of 12,000 tons a distance of six thousand sea miles at 15 knots is contained in 22 ounces of radium. The *Saxon* probably burns five or six thousand tons of coal on a voyage of approximately the same length. Again, M. and Mme. Curie have proved that radium actually gives out heat *, and it has been calculated that a small proportion of radium in the Sun would suffice to explain its present radiation. Other lines of argument tend in the same direction *.

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* Lord Kelvin has estimated the age of the Earth from the rate of increase of temperature underground. But the force of his argument seems to be entirely destroyed by this result.

Now we know that the Earth contains radio-active materials, and it is safe to assume that it forms in some degree a sample of the materials of the solar system. Hence it is almost certain that the Sun is radio-active also; and besides it is not improbable that an element with so heavy an atom as radium would gravitate more abundantly to the central condensation than to the outlying planets. In this case the Sun should contain a larger proportion of radio-active material than the Earth.

This branch of science is as yet but in its infancy, but we already see how unsafe it is to dogmatise on the potentialities of matter.

It appears, then, that the physical argument is not susceptible of a greater degree of certainty than that of the geologists, and the scale of geological time remains in great measure unknown.

I have now ended my discussion of the solar system, and must pass on to the wider fields of the stellar universe.

[To be continued.]

New Measurements of the Distance of the Sun.

[Continued from p. 354.]

I SUPPOSE that there will always be two opinions upon the question: Is the adopted value of the Solar Parallax to depend upon direct observation, or are the indirect determinations through the constant of aberration, the parallactic inequality, and the mass of the Earth to be allowed a weight in some proportion to their numbers? I take it that those of us who have determined the parallax by direct observations may not unnaturally look upon these indirect methods as interesting confirmations of our results, if they agree with it, while if they differ there must be something wrong with them. But in the absence of a direct determination of overwhelming weight, there must always be a feeling of uneasiness when one sees three or more results conspiring to deny the truth of one. And however that may be, it is certainly true that about the year 1898 there was a very general suspicion abroad that the value 8″·80 was too large.

At this moment there came a curious stroke of fortune. Dr. Witt, of the Urania Sternwarte, Berlin, was engaged in a photographic search for a minor planet which had long been lost. He failed, but found instead a minor planet for which one would willingly barter the remaining five hundred odd; a minor planet indeed, but moving in a most remarkable orbit, lying for the most part within that of Mars, very eccentric, considerably inclined to the ecliptic, and approaching the Earth on favourable occasions within about 15,000,000 miles. It was immediately recognised that here was a new opportunity for determining the Solar Parallax, and that the determination must be made at once or left alone for thirty years, for a comparatively favourable opposition was due in 1900 and no more good ones till 1930 and 1937. At the meeting of the Permanent Committee which directs the making of the Astrographic Chart and Catalogue of the whole sky, it was resolved to invite a great co-operation of observatories to make a combined onslaught on the problem. The suggestion was readily taken up, with an alacrity indeed which might almost have suggested that the observatories concerned had nothing to do and were glad of a job, an imputation which is immediately rejected when one finds that some of the most energetic participants were precisely those observatories that had their hands most full with the astrographic chart. By a cruel stroke of fate Sir David Gill at the Cape was compelled to remain a spectator of the work, for the planet came so far north that it was practically unobservable in the Southern Hemisphere, while in England we had the unique spectacle of a planet north of the zenith.

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* See W. E. Wilson, *Nature*, July 9, 1903; and G. H. Darwin, *Nature*, Sept. 24, 1903.

To discuss in any detail the circumstances of the apparition, and the way in which they can be utilized for a determination of the parallax, would take too long. But we may get a good idea of a fairly typical case by transplanting ourselves in imagination to the planet Eros on the evening of the 2nd of December, 1900, armed with an imaginary telescope ridiculously out of proportion to the real size of the planet, which is probably not more than 20 miles in diameter. The Earth is past inferior conjunction with the Sun, and appears as a crescent. The North Pole of the Earth is tilted towards us, and by the aid of this projection of the meridians and parallels of latitude we can with great ease trace the path of each observatory as it is carried round by the rotation of the Earth, and can measure from the scales the angular distance at any moment of an observatory from the centre, or the distance between two observatories; which angular distances as seen from the planet are the precise equivalents of the parallactic displacements of the planet as seen from the Earth.

In the programme of observations there was one novel and promising feature—the application of photography. With the exception of the Transit of Venus observations, in which its success was not striking, photography had not been previously applied in a determination of the solar parallax, for the very good reason that in 1889, at the time of the opposition of Victoria, there was practically only one telescope in existence which was capable of taking photographs for exact measurement, that pioneer photographic equatorial made by the brothers Henry at Paris. The fact that there were in 1900 eighteen photographic telescopes engaged in observing Eros shows how rapidly the equipment of astronomy has grown in the last few years. We were so fortunate at Cambridge as to have our new photographic equatorial just completed and made to work. (I may remark parenthetically that it took longer to make the machine work than to build it, for when one embarks upon a large experiment, and sets up an instrument, the first of its kind, built upon improved lines, one sets out upon a sea of troubles.)

The great advantage of the photographic method in such an undertaking must be sufficiently well known by you. It is of course this, that one is rendered very much more independent of continued fine weather. A photograph of the planet and the surrounding stars could be made in 2 or 3 minutes of actual exposure. Given an hour's break in the clouds, one could accumulate far more valuable material than could be obtained in a whole night's visual observation, for the photograph once secured could be measured at leisure, by day or on cloudy nights.

Throughout Europe the skies of that winter were far from clear. I had the pleasure at Cambridge of sitting up from dusk till dawn for nearly three months on end, and during that time had not half-a-dozen nights clear right through. Had I been making visual observations I should have done little; as things turned out I was able to get some 500 exposures. The programme was to get 4 exposures per hour throughout the night, making a number of exposures on each plate, and moving the plate a little between each exposure. The stars are then arranged in columns of fours; the images of the planet, owing to its rapid motion, are in echelon.

Now each exposure gives very accurately the place of the planet with reference to the group of stars around it, and for merely parallax purposes the ideal would be to have pairs of such photographs taken at the same instant at stations widely separated. By a very simple use of the measures of some ten or twelve stars suitably disposed about the planet, it would be possible to allow almost automatically for the differences of refraction, orientation, scale value, &c., which make the plates not immediately comparable, and to find at once the parallactic displacement.

We have been speaking of the displacement as very large, and so it is when compared with the displacements dealt with in previous determinations. But look at it this way. We saw that the Earth as seen from Eros subtended an angle of 53″. The scale of the Cambridge plates is such that, if we draw a circle having a diameter of a little over 1½ mm., we represent the apparent diameter on our plate of the Earth as seen from Eros; and within that small circle the whole of the parallactic displacement must necessarily lie. About a millimetre was the average displacement obtained in a favourable combination of observations, and when we consider that we are trying to measure that with a resultant accuracy of 1 in 1000, it does not seem so very great after all.

We have put the problem heretofore in its very simplest form. In actual fact the exposures at different stations were not simultaneous. Early morning observations made at Cambridge might be combinable with evening observations at Lick more or less simultaneous, or with evening observations 10 or 12 hours before at (say) Oxford; or they might have to stand alone. Any general method of utilizing all the results must secure the possibility of reducing each plate, so to speak, on its own merits, to allow it to contribute its quota, be it large parallactic displacement or none at all, to the general collection of equations of condition. This requires that we shall know the relative places of all those stars which are to be used as comparison stars for the planet, right along the whole track of the planet. And this derivation of a standard star system is by far the most delicate and difficult part of the whole work. One must start with a foundation of stars observed with the meridian circle, and fill in the fainter stars from the photographs themselves, taking care to provide at the same time the places of all those faint comparison stars which have been used by the visual observers. And all these places of stars must be tied together, so to speak, by the overlapping of the photographs, so that the system may run smoothly throughout its length. Absolute errors of zero there no doubt will be, and must be, but it is required that there shall be no sudden jumps in the errors exceeding one or two hundredths of a second of arc.

Now when one comes to face a problem like this one must enquire very carefully what is the real accuracy of the photograph. There is no doubt that the ordinary photographic telescope properly worked will repeat itself very well; it will take two plates of the same region which agree with one another excellently. But the question is,—How would two plates of the same region taken with different telescopes agree? We know that individual observers have peculiarities of their own which they can repeat almost *ad infinitum.* Does a photographic telescope do the same, or has it at last conquered that bad habit of idiosyncrasy which has made so much trouble in all refined astronomical work of the older kinds? When we started on the Eros campaign there was practically no information to be obtained upon this point. Almost all the photographic telescopes at work had been engaged upon their own zone of the chart, and almost nothing was known of how the results from different instruments would combine. But in our parallax problem this question is fundamental, and must be answered as soon as possible. I therefore ventured to propose to myself to undertake the reduction of a small section of the photographic results, for a period of 9 days in November, 1900, having before me two objects:—Firstly to discover how far it is possible to combine photographs taken at different observatories, how far, in fact, photographic telescopes are giving really accurate results or merely reproducing their own errors; secondly to obtain a provisional value of the solar parallax, with a probable error if possible as small as that found by Sir David Gill with the heliometer, and to find out provisionally whether Eros was going to confirm that result or to join in the secession from the adopted value.

Perhaps I may venture to think that the results of this enterprise have been in some measure successful. I found that as a general rule the results from different telescopes do not combine directly as well as could be hoped, and that there are many precautions which must be taken in using them, if we are to avoid serious systematic error and a ruination of the parallax determination. But I believe that it is possible to avoid these difficulties, and that the photographs properly treated will give a determination of the parallax of far greater accuracy than has hitherto been obtained. I found also that the 300 exposures in that period of nine days, contributed altogether by nine different observatories, gave a value of the solar parallax, $8''\cdot797 \pm 0\cdot0047$, which is in such nice agreement with Gill's $8''\cdot802 \pm 0\cdot005$ that one may feel in one's heart (though of course must never express the feeling so prematurely as this) some hope that, in adopting $8''\cdot80$ as the official value of the solar parallax, the conference of 1896 was not so wrong as some people have been prepared to believe.

From Heliometer Observations of Victoria, Iris, and Sappho	$\pi = 8''\cdot802 \pm 0''\cdot005$
Corresponding Distance of Sun	92,875,000 miles.
From 295 Photographs of Eros	$\pi = 8''\cdot797 \pm 0''\cdot0047$
Corresponding Distance of Sun	92,928,000 miles.

I cannot refrain from calling your attention to a by-product of this work which has for me a singular interest, because it seems to exhibit in a favourable light the accuracy which we may obtain with these photographic methods. After Eros had been under observation for some time it was discovered that its light was varying in a short period which was at first thought to be $2^{h}\ 38^{m}$. Afterwards it was found that the alternate minima of light was unequal, and that the true period should be reckoned as $5^{h}\ 16^{m}$, two equal maxima and two unequal minima being included within that space of time. The variation appears to be continuous, without sensible pause, which precludes the idea that the planet is double and that the minima are due to eclipses of one body by the other. We must find some other cause. There are two which suggest themselves quite naturally — irregularity of shape and irregularity of surface brightness. For our purpose the important point is this, that either of these causes might produce an apparent oscillation in the place of the planet. To discover if this were so, I grouped the residuals in my equations of condition according to their epoch into eight columns, corresponding to successive eighths of the whole period of $5^h\ 16^m$, and took the means for each column. If there were a sensible oscillation in a period of $5^h\ 16^m$, these would lie on a sine curve. They obviously did not. But notice that if we add together the 1st and 5th, the 2nd and 6th, and so on, that is to say if we search for the half period of $2^h\ 38^m$, we get quite strong evidence of periodicity. Now the semi-amplitude of the oscillation is only 0″·03, a quantity so small that one cannot but feel doubts as to its reality. At first I was myself inclined to disbelieve; but when a new distribution of the residuals, starting from a different zero of time, gave a similar periodicity, it began to look as if there were something in it. The more I look at it, the more I believe that it is a reality; and that the photographs have shown themselves accurate enough to detect an inequality of 0″·03, corresponding to a shift of 5 miles at a distance of 25,000,000—by far the smallest inequality in the motion of a planet ever brought to light by observation.

It is this circumstance that encourages one to believe in the accuracy of the photographs. There are altogether 10,000 separate exposures of the planet which will within the next few years be measured and made available for discussion. If 300 give a P.E. of 0″·005, what will 10,000 give, added to 6000 or 7000 sets of visual observations? It would be going too far to apply the simple rules of probability and say a good deal less than 0″·001. But I fully believe that, if this great array of observations is ever submitted to complete discussion, the probable error of the result will not be much above 0″·001. And supposing that it should support with its greater weight the value 8″·80 which has been assailed, I believe that we should be justified in saying that the Solar Parallax *is* 8″·80, and in maintaining the proposition that the determination of the Solar Parallax is a problem of geometry and celestial surveying, and that upon the sponsors of the indirect methods lies the onus of showing cause for their disagreement.

This opens up an interesting prospect. Suppose that in course of time there should come to be a clear and definite agreement among the values found for the constant of the aberration of light, and that its value was (let us say) 20″·54, corresponding, as this table shows, to a parallax of 8″·77, not 8″·80, on the assumption at least that the velocity of light is exactly determined, as it seems to be, and that the simple theory of aberration is correct.

Relation between Solar Parallax and Constant of Aberration

Ab.	π.
20″·46	8″·808
·48	·799
·50	·790
·52	·782
·54	·773
·56	·764

And suppose that by that time we are prepared to say quite definitely that the geometrical value is not 8″·77 but 8″·80. The most obvious solution of the difficulty would be to conclude that the simple theory of aberration is not true, and to hand over the problem to the mathematical physicists, who might in the result find that a definite geometrical determination of the Solar Parallax had provided just the criterion which they required to settle certain vexed questions in dynamics.

Again, should further investigation confirm the conclusion that 8″·76 is the only value of the Solar Parallax which will reconcile the existing theory of the motion of the planet with the observed value of the constant of gravitation, it may be that the contradiction between the direct and the indirect methods will at last enable the dynamical astronomers to lay a finger upon that flaw which exists somewhere or other in the theory, and makes it impossible to say at the present time that all the motions of the solar system can be completely explained.

I have ventured to point out that the determination of the Solar Parallax is a problem of wide interest, since it throws upon so many different people the task of keeping up their particular end against an attack whose accuracy is gradually becoming more and more deadly. The dimensions of the Earth, as obtained by geodetic operations, are necessarily beyond the reach of any criticism derived from Solar Parallax results.

Equatorial Radius of Earth.

	Miles.
Méchain and Delambre, 1799	3961·74
Airy, 1830	3962·82
Everest, 1830	3962·67
Bessel, 1841	3962·76
Clarke, 1858	3963·31
,, 1866	3963·28
,, 1878	3963·37
,, 1880	3963·29

Extreme range of these determinations is only 1½ in 4000.

But it is interesting to speculate whether astronomers will ever be in the position to say:—“We have now determined the Solar Parallax in seconds of arc to a higher degree of accuracy than that of the measurement of the Earth,” and to call upon geodesists for better results. I can conceive only one direction in which we may be able to worry the successors in your Corps of Everest and Clarke. Is the form of the equator a circle or an ellipse? I believe that there is some slight evidence for ellipticity, and that it has been put as high as one in three thousand. If that is so, it is just barely possible that we may have to introduce into the computation of the parallax factors for different observatories a term depending upon the shape of the equator. But I confess that this prospect is remote, and that for many years in all probability geodesists, who achieve accuracies of one in a hundred thousand and even talk of one in a million, will be able to take a serene view of the labours of astronomers to arrive at the distance of the Sun to one part in ten thousand.

The Total Solar Eclipse of August 30, 1905.

During the few weeks that have elapsed since the eclipse there has been opportunity to collect more precise information of the amount of success of the different expeditions than was contained in the telegrams hurriedly published in last month's number. For complete knowledge of the results obtained, it is necessary to wait for the meeting of the Joint Eclipse Committee of the Royal and Royal Astronomical Societies, which will be held in the rooms of the former Society on October 19; but meanwhile it may be well to tabulate the facts already known, arranging the observing-stations in the order in which the Moon's shadow visited them.

As to Labrador, a telegram from Mr. Maunder reported "Sky quite cloudy, no results"; and, so far as is known, this sums up the situation on the western side of the Atlantic.

At Burgos, the first station in the eastern hemisphere, there was a large concourse of observers, which included the King of Spain; the weather conditions seem to have varied considerably with the locality, for Mr. Evershed, who had set up a very fine prismatic camera near Burgos, telegraphed "Thick clouds, no results"; but some observers seem to have had unexpected good fortune. From a report in a Southport paper by Mr. E. D. Benson, it seems that at twenty minutes to one the sky was completely overcast and rain was falling, but, almost miraculously, five seconds before totality the Sun passed into a gap between the clouds and the corona and prominences were well observed. Mr. Benson remarks that the temperature dropped some nine degrees during totality, that Mercury and Venus were visible, but general observations of stars were impossible owing to clouds in the sky, and that shadow-bands were seen flickering along the ground after totality. The same report is also responsible for the statement that the night before the eclipse the price for bed and breakfast in Burgos was four pounds in English money.

On the eastern coast of Spain, Father Cortie was at Vinaroz, assisted by Mr. Adrian Liddell, Mr. L. Cafferata, and Mr. G. de Aquilera. Father Morford, of Saltash, was also of the party. Father Cortie says: "In spite of light clouds that passed over the Sun at the beginning of the eclipse, we have been on the whole very successful. We have six beautiful pictures of the corona taken with the long focus (19 ft. 4 in.) 4-inch lens belonging to the

Royal Irish Academy. The scale is 2·1 inches to the solar diameter. We also have four plates giving images of the corona and the spectrum of prominences and corona on the same plates taken with the direct image and first order spectrum of a Thorp transmission grating and a 3-inch quartz lens." At the Observatory of Tortosa, north of Vinaroz, there was a large party, besides the observers attached to the establishment, under Father Cirera. The sky was clear there for about a minute and a half during totality, and the observers obtained four good photographs of the corona, several fine drawings, and a great stock of electrical, magnetic, and meteorological data. At the foot of the mountain at Tortosa the French official party under M. André had clouds, and at Castellon, also on the east coast, but south of the central line, where Prof. Callendar was in charge of one of the official parties, and where Prof. Alex. Herschel and others had located themselves, totality was entirely obscured by clouds, but the first and last contacts were observed in clear sky, and good records of radiation and temperature were obtained.

We have received a careful account from Prof. Moye, of Montpellier, who was in this neighbourhood, which is given later verbatim.

Leaving Spain, the next station was on the Orient steamer 'Ortona,' situated on the central line in the Mediterranean, near the Spanish coast, from which an excellent view of the eclipse was obtained. Prof. Joseph Larmor, who was on board, communicated a valuable account of his naked-eye observations to *Nature* (September 7). He notes that the streamers were not all radial, and that the most prominent were those at 45° from the vertex to the left, and a long thin streamer 67½° from the lowest point to the left. The lightness of the eclipse was marked and disappointing, but Venus and Regulus were seen, and the whole black disc of the Moon was visible shortly before totality. Shadow-bands—dark strips about 6 to 8 inches wide, distance apart about 18 inches—were seen after totality rippling along the deck a little faster than could be followed by the eye. They were parallel to the strip of the Sun after totality, and travelled in direction of shadow.

In Majorca the clouds were very local. In the town of Palma, and at Sir Norman Lockyer's camp about a mile to the west, the first contact and almost the whole of the partial phase was observed in clear sky, but at 2 minutes past 1 G.M.T. clouds gathered and covered the Sun: these lasted with varying thickness until totality began, and all the total phase was seen under a veiling of cloud. Nevertheless, the inner corona was seen as a brilliant circle of silvery light, shading here and there to blue, and the prominences were quite visible. Neither Mercury nor stars were seen * because of the cloud which covered them, but Venus was seen by several persons. Some who looked carefully for shadow-bands saw them but vaguely if at all, although the preliminary report from Sir Norman Lockyer's camp says that the observers there gained a great deal of information about this phenomenon. This same report, whilst lamenting the clouds, states that some results were obtained, both spectroscopic and photographic—Mr. F. McClean having obtained a fine photograph of the corona with sharp detail and good extension, and Mr. Butler an excellent picture of the lower corona, the solar diameter being 8⅛ inches. In some other parts of the island, not far from Palma, observers were more fortunate. A party of Jesuit Fathers under Father Algué, the Director of the Manila Observatory, had a fine sky, and secured good photographs and drawings.

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* The statement in last month's number that Mercury and the brighter stars of Leo were seen at Palma was due to an error in the code telegram.

At Guelma, in Algeria, Mr. Newall was very fortunate, as may be seen from his telegraphic report printed on p. 360 of our September number. Specially it is to be noted that he saw long streamers extending towards Mercury, *i.e.* to the S.W., over three degrees. We must not anticipate the report of the observing party at Sfax, but we are in a position to state that the photographs of the corona and the spectrum plates taken by the Astronomer Royal's expedition are coming out well, although the weather conditions were not all that could be desired.

Prof. David Todd was at Tripoli, where he says "the conditions, except for the intense heat, which we momentarily feared would snap our great cameras, were the most nearly perfect imaginable at a sea-level station." So far as telescopic work was concerned, Prof. Todd devoted himself to photographing the corona, which he thinks was less impressive than other coronas, but besides this his party made carefully-organized observations of shadow-bands at request of Mr. L. Rotch, the American meteorologist. Of these observations he writes in *Nature* of September 14:—"These bands were seen as early as ten minutes before totality, and had many remarkable and pronounced peculiarities. They were wavering and narrow, moving swifter than one could walk, at right angles to the wind, their length with it, and waxing and waning five times during the eight minutes preceding totality."

At Assuan, where Russian, American, and British expeditions were located, the weather left nothing to be desired. Prof. Hussey, from the Lick Observatory, developed his plates on the spot, and reports them all as satisfactory. The plates of the British expedition show a considerable amount of corona. The method of obtaining pictures in polarized light by reflection from the face of a glass prism was very successful, and it cannot be doubted that the measurement of these plates will yield results of great value. The American party at Assuan made some interesting observations of shadow-bands. Their velocity was quite moderate, and the direction of motion was the same before and after totality, agreeing with that of the wind, but there was almost a dead calm at Assuan.

Account by Prof. M. Moye.

I observed this magnificent eclipse at Alcala de Chisvert, a little town on the oriental coast of Spain. The first contact was seen in a perfectly clear sky, but some minutes after, two or three cumuli went from W. and slowly made their way before the Sun. I was again in despair, but at the eventful moment clouds were absent, and totality displayed its beauties without interference, except for a light and transparent vapour at the end, of which I shall say a word later.

In order to secure good observations my programme was very limited, and I had plenty of time to fill it without hurry. I restricted myself to the observation of: (1) the shadow-bands, (2) the corona, (3) the general aspects of the eclipse.

The Shadow-bands.—In 1900, at Elche, I had a good view of this phenomenon, and I was eager to see again these enigmatic bands. I saw them three minutes before and three minutes after totality, being unable to catch them during the total phase. They were greyish ribbons, not black, tolerably distinct, and very wavy. I noticed their direction from S.W. to N.E., and their motion perpendicular to this, *i. e.* from N.W. to S.E., both before and after totality. The wind, moderate, was blowing from S.W., and clouds drifted from W. or N.W.

On the soil, white and smooth (the platform of the station), I had some rods with metric gradations; I estimated the width of the bands at two inches and their distance apart at three or four inches. The motion was slow, difficult to observe exactly, perhaps one or two inches per second. The shadow-bands reminded one of a rope tied at one end and made to wave by the hand at the other end.

For observations of the corona I used only a good opera-glass. I sketched carefully the coronal streamers, and I had time enough to compare my drawing with the actual Sun. The Sun was encircled by a white silver ring, very bright, almost dazzling on the limb, but fading away softly in the darkness of the sky. Around this inner corona an intricate system of streamers of a pearly hue, and seeming on the whole as an irregular star with six or seven points. Long rays were diverging from the solar poles, a perfect contrast with the equatorial wings of the 1900 eclipse.

Even to naked eye two groups of prominences were evident—magnificent rosy flames on the white corona. The brightest group was at N.E. and the other at S.W., almost diametrically opposite. All the coronal streamers were more or less of an ogival form, or "angel's wing," as noticed in previous eclipses. Their bases were broad, and they were tapering and melted away very gradually. Three of the streamers are worthy of a special notice.

The N.E. ray was exactly hanging over the beautiful prominence seen at its root. It seemed an extension, or, better, a sequence of the solar eruption. However, the second group of prominences (at the S.W. limb) did not disturb, apparently, the coronal filament.

The N.W. ray was one of the longest, about one and a half lunar diameters (from the limb). Curiously enough, its northern boundary was very sharp, even on the lunar limb, and in the inner corona a dark rift was conspicuous, seen even with the naked eye.

The southern streamers went as far as two lunar diameters (from the limb). There the corona displayed its greatest activity. These streamers were complex, apparently double, as formed by two ogives overhanging.

On the whole, the corona was decidedly brighter than in 1900. Its tolerably regular features, its stellar form, the polar activity, were just things to be expected in a maximum year. At first glance I was struck by the perfect likeness of the actual corona with the predicted drawing of the astronomical text-books.

The general appearance of the eclipse was perhaps less interesting than that of 1900. At totality light was surprisingly intense. I did not find any difficulty in sketching my drawing or in reading small letterpress. The features of the landscape, the details of a railway-engine, the divisions of a watch—all were seen very easily. The eclipse of 1900 is recorded as a bright one; for myself I believe that of 1905 yet brighter, connected perhaps with the greater activity of the solar envelope. Owing to the general illumination, decidedly lighter than a Full Moon night, I was unable to see any star or planet, with the exception only of Venus. Sky was grey, not dark, rather a neutral tint, especially in the north, with a yellow fringe in the south. Clouds were also greyish, but without any abnormal appearance. On the landscape there was the same greyish hue. It reminded one of an ashes rain or of a shroud, and the prevailing impression was of dullness and sorrow.

The eclipse wind was very noticeable, coming to a perfect calm some minutes after totality. In the shade the fall of temperature was about 5° F. In the heights of the atmosphere the fall must have been greater, for at totality a thin vaporous veil (alluded to before) sprang around the Sun. It was not a cloud, being motionless, and unseen before and after totality, rather a sudden condensation of the aqueous vapour by the coolness of the coming shadow.

Shadow-bands.

It will be noticed that in the above summary care has been taken to give full details of the observations of shadow-bands, which are long dark bands, or sometimes lines of patches, separated by white spaces, which are seen on the ground or sides of buildings just before and just after the total phase of an eclipse moving rapidly. Observations hitherto made have been quite discordant, and the cause of the phenomenon is not known with certainty, although there are theories. One of these assumes that the bands are a diffraction effect when the Sun becomes a narrow strip; another theory is described in the following extract from an article on the subject by Prof. Cleveland Abbe, which (written in 1887) appeared in the American *Monthly Weather Review* for 1900, May:—

"Meteorology has perhaps not much to expect from observations of the barometer and the wind during a total solar eclipse, but it has considerable interest in the shadow-bands. It seems quite plausible that the explanation of these is to be found in the interference of two pencils of sunlight that have respectively passed through adjacent portions of air of slightly different densities. This should not be called a diffraction phenomenon, though it does occur when a thin sheet of light from the edge of the Sun passes the edge of the Moon at the moment preceding totality. Undoubtedly such a slender beam of light may cause diffraction phenomena, but, if so, the diffraction bands would necessarily move onward over the Earth's surface with the same relative speed as that of the Moon and the Earth, namely approximately a mile a second, whereas the observed shadow-bands have a velocity of only a few feet or yards per second. On the other hand, the bands may be considered as phenomena of interference of rays of light slightly inclined by reason of the irregular refractions in a non-homogeneous atmosphere, and they must therefore have such characteristics as are impressed upon them by the condition of the atmosphere at the time; their horizontal movement must correspond nearly with that of the winds and upper currents in in the atmosphere. In fact, we need not necessarily speak even of interference phenomena. Every small mass of descending dense air constitutes a rough sort of lens or prism; the beam of light that passes through it must be deflected, and the atmospheric mass casts a shadow on the ground, like a lens or prism of glass. Such phenomena may be seen when the air is very much disturbed even in ordinary full sunlight, but become much better defined when the Sun's disc is reduced to a slender crescent, as during the few seconds preceding and following totality."

International Union for Co-operation in Solar Research: Meeting at Oxford, 1905 *September* 27–29.

By the kindness of the Warden and Fellows the Second Meeting of the "International Union for Co-operation in Solar Research" was held at New College. The proceedings were opened on Wednesday, September 27, by *Prof. Turner*, who briefly alluded to the St. Louis Meeting, and expressed the hope that some good might be done by a few days' intercourse in the quiet of Oxford. In the absence of the Warden, the Sub-Warden, *Mr. Matheson*, welcomed the Conference. He referred to the historic connexion of New College with scientific research, particularly astronomy, and spoke of the general value of the meetings of men of different nationalities in promoting good feeling between the different countries in addition to their scientific importance.

Prof. Schuster, on behalf of the Conference, returned thanks to the Warden and Fellows. He went on to say that since the last Meeting necessarily the business of the Association had rested with the Executive, and that body had acted on the principle that "it was better to do something irregularly than nothing regularly."

He proposed *Sir William Christie* as President for Wednesday, and *M. Janssen* as Hon. President, and *Prof. Turner* as President for Thursday. This was seconded by *Sir Norman Lockyer.*

At the Meeting at 10.45 *Sir William Christie* presided, M. Janssen and Prof. Schuster being on the platform with him. *Prof. Kayser* proposed, and *MM. Perot* and *Fabry* seconded, the following resolutions on new determinations of wave-lengths:—

1. The wave-length of a suitable spectroscopic line shall be taken as the primary standard of wave-length. The wave-length of this line shall be fixed permanently and thereby define the unit in which all wave-lengths are to be measured. This unit shall differ as little as possible from 10^{-10} metres, and shall be called the Ångström.
2. Secondary standards are required at distances which should not be greater than 50 Ångström units apart. These secondary standards should be referred to the primary standard by means of an interferometer method. The source of light should be obtained by means of an electric arc of from 6 to 10 ampères.
3. A Committee shall be appointed to select the standards and to organize the determination of their wave-lengths in terms of the primary standard in at least two independent laboratories.
4. The same Committee shall be charged with the selection of tertiary standards which shall be at distances of from 5 to 10 Å. The wave-lengths of these tertiary standards are to be obtained by interpolation with the help of gratings.

The resolutions were carried, and further details left to a Committee.

The second resolution was moved by *Prof. Hale* and seconded by *M. Deslandres*—"That a Committee be appointed to draw up resolutions to be submitted to the Meeting to-morrow."

Prof. Hale said that a complete programme for solar research would involve:—

(i) Solar photographs on, say, an 8-inch scale for the study of their general positions on the Sun's surface and for statistical enquiries.
(ii) Enlarged scale photographs like those of M. Janssen.
(iii) Photographs in calcium light with the spectroheliograph.
(iv) Photographs in the light from other metals.
(v) Spectra of the K line in different parts of the Sun.
(vi) The spectra of sun-spots, the widening of the lines, radiation from sun-spots, &c.
(vii) Visual observations of chromosphere and spots.

He pointed out that there was urgent need for co-operation and an ample field for research in these many lines.

M. Deslandres, *Mr. Newall*, *Prof. Belopolski*, and *Sir Norman Lockyer* were all agreed that co-operation was absolutely necessary, and need not be in the least degree detrimental to work requiring individual initiative.

Prof. Fowler and *Father Cortie* spoke of the advantages to be obtained by a scheme of co-operation in work on the spectra of sun-spots.

At the Meeting in the morning of Thursday, September 28, at which *Prof. Turner* took the Chair, the following resolutions dealing with Solar Radiation were submitted :—

1. In order to secure uniformity it is desirable that observations on the intensity of solar radiation in different localities shall be made as far as possible with the same type of instrument.
2. That for the present Ångström's pyrheliometer be adopted as the standard instrument.
3. That it is desirable to obtain accurate comparisons between the records of Ångström's pyrheliometer and other standard instruments: and that Mr. Abbott, Prof. Callendar, Mr. W. A. Michelson, and Mr. W. E. Wilson be asked to assist the Union in this work.
4. That for the determination of the possible changes in the solar-radiation power, it is desirable to secure measurements of the intensity over limited ranges of the spectrum, which are not affected by absorption due to ozone, aqueous vapour, and carbonic acid.
5. That a Committee be appointed to draw up a scheme of co-operation, and proposals for the reduction of observations. That the Committee be requested to communicate the scheme and proposals to the Executive Committee, with a view of initiating a system of observations according to the scheme.
6. That the Union recognises the great importance of measurements by direct photography, as well as by other methods, of the relative intensities of radiation emitted by different parts of the solar surface, and desires to include such measurements in the subjects to be dealt with by the Union.

The first five of these were brought forward by *Prof. Schuster*, the sixth was added by *Sir William Christie*.

Prof. Ångström read a paper describing his pyrheliometer, and *Prof. Julius* spoke in support of the resolutions generally.

Mr. W. N. Shaw explained that the International Meteorological Conference had adopted the resolutions numbers 1 and 2, and urged that the same instruments and methods should be continued for a long time.

Sir W. Christie suggested that attempts should be made to photograph the umbræ of spots on a large scale and isolated from the surrounding parts of the Sun's disk.

Prof. Hale explained Abbott's bolometric work on Sun-spots, showing the difference in radiation when different lines were taken.

Prof. Hull and *Prof. Belopolski* also spoke on the resolutions.

Subsequently the following resolutions, which were the result of the deliberations of a Committee consisting of *Sir Norman Lockyer*, *Prof. Hale*, *M. Deslandres*, *Prof. Belopolski*, and *Prof. Wolfer*, after being amended at the Meeting were proposed by *Sir Norman Lockyer* and agreed to:—

1. Co-operation is desirable in the various branches of Solar Research; such as visual and photographic observations of the solar surface, visual observations of prominences, and observations of the solar atmosphere with spectroheliographs of various types.
2. When an institution has collected and co-ordinated results from various sources, members of the Union shall be requested to place their observations at the disposal of the said institution.
3. In the case of investigations which have not yet been thus collected and co-ordinated, special Committees nominated by the Union shall be charged with the work of preparation and organisation. That these Committees be requested to communicate the scheme and proposals to the Executive Committee, with a view of initiating a system of observations according to the scheme.
4. It is proposed forthwith to organize such co-operation in two branches of research:
    - (*a*) The study of the spectra of sun-spots.
    - (*b*) The study of the records, by means of the H and K light, of phenomena of the solar atmosphere.
5. The Union lays special stress on the fact that, notwithstanding the obvious utility of co-operation in certain cases, individual initiative is the chief factor in a very large number.

It is as much the duty of the Union to encourage original researches as to promote co-operation.

There was some discussion on these resolutions leading to some amendment, in which most of those present took part. At the instigation of *Sir William Christie*, the words "special Committees nominated" were substituted for "a Committee specially nominated" in resolution 3, and the second part of this resolution was added in the course of the discussion. *Prof. Belopolski* drew attention to differences of kinds of work—some in which co-operation would be helpful, others depending on individual initiative. *Sir Wm. Christie* asked how far co-operation in work other than that with the spectroheliograph was desirable, and *Father Cortie* thought the danger of overlapping was avoided by co-operation.

Prof. Fowler proposed that a Committee should be formed to arrange for spectroscopic observations of sun-spots.

Prof. W. W. Campbell instituted a discussion on the spectroscopic determination of solar rotation, and *Dr. Halm* described the instrument used by him at Edinburgh for this purpose.

The concluding Meeting was held on the morning of Friday, Sept. 29, when the proceedings were mainly of a formal character. It was resolved to accept the invitation of M. Janssen to Meudon in September 1907. Professors Schuster and Hale were elected members of the Executive Committee, the first-named being Chairman. That the Central Bureau should be at the University of Manchester and the Computing Bureau at the University Observatory, Oxford, under the direction of Prof. Turner, and Committees were elected to deal with the four subjects:—(1) Standards of wave-length, (2) Solar radiation, (3) Co-operation in work with the spectroheliograph, (4) Co-operation in the work on spectra of sun-spots.

On Thursday the Delegates were entertained to dinner by the Royal Astronomical Society Club in the Hall of New College, and in the evening of Friday, September 29, they attended a reception given by the President of the Royal Astronomical Society (Mr. W. H. Maw) and Mrs. Maw at their house in South Kensington. On Saturday, September 30, they visited Cambridge, at the invitation of Sir Robert Ball and Mr. Newall.

List of Delegates and Visitors present at the Meeting.

Name.	Representing.
Prof. K. ÅNGSTRÖM	Acad. Sci. (Stockholm).
Prof. A. BELOPOLSKI	Acad. Sci. (St. Petersburg).
Mr. F. A. BELLAMY (Oxford).	
Prof. W. W. CAMPBELL	Nat. Acad. Sci. (Washington).
Sir WILLIAM CHRISTIE	Royal Society (London).
Fr. CIRERA	Ast. Soc. of France.
Fr. CORTIE	Royal Ast. Soc. (London).
Cte. DE LA BAUME PLUVINEL	Ast. Soc. of France.
M. H. DESLANDRES	Ast. Soc. of France.
Mr. H. DUFFIELD (Manchester).	
Mr. F. W. DYSON (Greenwich).	
Prof. W. S. EICHELBERGER.	
M. FABRY	Physical Soc. of France.
Prof. A. FOWLER	Royal Ast. Soc. (London).
Prof. G. E. HALE	Nat. Acad. Sci. (Washington).
Mr. J. HALM (Edinburgh).	
M. HANSKY	Acad. Sci. (St. Petersburg).
Major E. H. HILLS	Royal Ast. Soc. (London).
Prof. G. F. HULL.	
M. J. JANSSEN	Acad. Sci. (France).
Mme. JANSSEN.	
Mdlle. JANSSEN.	
Prof. W. H. JULIUS	Acad. Sci. (Amsterdam).
Prof. H. KAYSER	German Physical Soc.
Prof. LITTELL.	
Sir NORMAN LOCKYER	Royal Soc. (London).

Name.	Representing.
Dr. W. J. Lockyer	Solar Commission of Internat. Meteorolog. Comm.
Mr. Georges Millochan.	
Mr. H. F. Newall	Royal Soc. (London).
M. Perot	Physical Soc. of France.
Mr. H. C. Plummer (Oxford).	
Prof. A. Schuster	Royal Soc. (London).
Mr. W. N. Shaw	Internal Meteorol. Comm.
M. Stefanik.	
Prof. H. H. Turner	Royal Ast. Soc. (London).
Prof. E. Weiss	Internat. Assoc. Acad.
Mr. Horace White (New York).	
Mr. W. E. Wilson	Royal Ast. Soc. (London).
Prof. Wolfer.	

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Eclipse supposed to be referred to by Tertullian.

Gentlemen,—

In the address by Tertullian to Scapula, Proconsul of Africa, which is, in fact, a remonstrance against the persecution of the Christians, he speaks of various calamities and occurrences which appeared to indicate the divine wrath on the cruelties perpetrated, and one of them has often been supposed to refer to an eclipse of the Sun, total, like that of last month, in North Africa. His words are (c. iii.):—

> Nam et sol ille in conventu Uticensi, extincto pene lumine, adeo portentum fuit, ut non potuerit ex ordinario deliquio hoc pati, positus in suo hypsomate et domicilio.

Now the question is, if this were really an eclipse, what was its date? The address is thought to have been written early in the reign of Caracalla, who bore the name of Antoninus, because Severus is mentioned (c. iv.) as the father of Antoninus, presumed to be the reigning emperor. Severus died in A.D. 211, and Caracalla became sole emperor by the murder of his brother in 212. The eclipse of the 13th of March, A.D. 210, has been suggested, but the suggestion must have been a mere guess without examination, for that eclipse was total only in North-eastern Asia, and not even visible in Europe or Africa. That on the 14th of May, A.D. 207, was annular in Central Europe, and only a large partial one in North Africa, so that its conditions also do not apply, besides which it was four years before the death of Severus, and Tertullian is evidently speaking of some comparatively recent phenomenon.. The eclipse of the 3rd of June, A.D. 197, was indeed central (apparently annular and almost total) over Tunis and its neighbourhood and the eastern Mediterranean, but was far too much a thing of the past to be alluded to by Tertullian in his argument. This might perhaps apply to the eclipse of the 14th of

August, A.D. 212, which was total in the early morning in Italy and very large in North Africa. But on the whole it seems to me that the opinion of the late Prof. Salmon is to be preferred. In treating of the subject in the 'Dictionary of Christian Biography' (under "Scapula," in vol. iv. p, 591), he expresses the view that no eclipse was intended, but a sudden darkness caused by a dust-cloud or storm-cloud whilst the assembly at Utica was in session. He thinks, too, that Tertullian's subsequent remark, "Habetis astrologos," shows that it was known that no astronomical eclipse was taking place at the time. Moreover, even if Tertullian had had in his mind the eclipse of A.D. 197 (fourteen or fifteen years before the date of his writing), he would hardly have spoken of it as confined to the "conventus" or district of Utica, as it would have been total also in Mauritania and Numidia and several of the Mediterranean islands. I should add that my attention was drawn to this eclipse by Mr. P. H. Cowell. Yours faithfully,

Blackheath, 1905, Sept. 9. W. T. LYNN.

The Obscuration of "Bridges" in Sun-spots.

GENTLEMEN,—

With reference to Mr. Maw's letter in your August number, the following details taken from photographs obtained here may be of interest. Excellent photographs are available for the period July 12–16, but after that there was a spell of bad weather and the plate for the 17th is poor. On the 12th a bright streak is shown at the edge of the umbra, which is apparently the beginning of the "bridge." By the next day the bridge is seen quite clearly, and, as far as can be judged without detailed measures, the apparent change in position is due to the increase of the umbra. On the 14th, at $5^h\ 50^m$ G.M.T., the bridge is very distinct; and on the 15th, at $3^h\ 25^m$, it was almost equally distinct and of the same shape. On the 16th, at $2^h\ 40^m$, the two ends remained, but the central part had disappeared. On the 17th the two ends were still visible, but they had changed somewhat in shape. A plate taken on the 20th shows no trace of the bridge.

The spectroheliograms taken in H on the 14th, 15th, and 16th show the bridge well, and of a shape similar to that shown in the ordinary photographs; but the spectroheliogram of the 17th shows two bridges—the old one and a new one almost exactly parallel to it—both of which cross the umbra completely.

Yours faithfully,

The Observatory, Kodaikanal, C. MICHIE SMITH.
28th August, 1905.

Velocity of a Planet at any Point.

GENTLEMEN,—

Dr. Simon Newcomb, in his 'Reminiscences of an Astronomer,' remarks that in the early part of the last century only in Germany was astronomy pursued with any intensity of purpose. Nor is this an isolated opinion. It behoves us, therefore, to claim all the credit rightfully due.

In this belief I ask you to spare me half a page to show that the very elegant theorem concerning the linear velocity of a planet in its orbit, commonly known as Van der Kolk's theorem (*Astr. Nach.* 1426, 1863, July 14), is really due to our own Dr. Whewell, as he published it, 40 years before Van der Kolk, in his 'Treatise on Dynamics' (8vo, Cambridge, 1823), p. 38, as follows:—

"Cor. 2. In the first case (the ellipse)

$$\frac{1}{2a}=\frac{1}{D}-\frac{V^2}{2m}; \qquad \therefore\ V^2=2m\left\{\frac{1}{D}-\frac{1}{2a}\right\};$$

therefore V is the velocity acquired by falling from a distance $2a$ to a distance r.

"Hence $2a$ is the distance from the centre at which a body must begin to fall, so that when it reaches the curve, it may have the velocity of the body in the curve; and this distance is the same for every point in the curve."

That is so clear as not to admit of argument, yet both Prof. Young in his 'Text-book of General Astronomy'* and Dr. F. R. Moulton, of Chicago, in his 'Introduction to Celestial Mechanics' (New York, 1902), p. 139, wrongly ascribe the theorem to Van der Kolk and ignore Dr. Whewell, whose prior discovery is incontestable.

Not every theorem is "*made in Germany.*"

Grant me space, then, in which to claim posthumous justice for Dr. Whewell.

Yours faithfully,
Frank Robbins.

93 Aldersgate Street, E.C.
1905, Sept. 7.

Systematic Error in Transit Observations of Jovian Spots.

Gentlemen,—

I have read the Rev. T. E. R. Phillips's article with reference to this subject in the current number of the 'Observatory' with a great deal of interest. The figures which he gives are certainly suggestive of the observations having been affected by some kind of systematic error, though it is by no means impossible, or perhaps even improbable, that there may have been a certain rhythm in the minor variations in the motion of the Red Spot Hollow, having a rough relationship to a period of about 13 months †. It also seems conceivable that there may have been something peculiar about the *appearance* of the Hollow that might give rise to the observed differences.

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* 1888 Edition, p. 266; 1898 Edition, p. 288.

† The diagram on p. 346 might possibly give rise to a false impression, because the dotted lines representing the results of the individual oppositions are drawn without any intermediate spaces. It should be remembered that a considerable interval of time really separates the observations of one opposition from those of a preceding or succeeding one, so that all the observations are capable of being satisfied without any break by a slightly undulated form of curve.

As a slight contribution towards the elucidation of the question I give below, in a similar form to that adopted by Mr. Phillips, the results obtained here during the past few years, not for the Red Spot Hollow, but for the Red Spot itself.

Date.	Long.	No. of Rotations.	Deduced Rot. Per.	Intermediate Rot. Per.
	°		h m s	h m s
1899 March 13	27·0	229	9 55 42·6	9 55 41·7
1900 April 20	37·8	287	9 55 42·3	9 55 41·2
1901 August 21......	45·2	229	9 55 40·9	9 55 40·0
1902 September 25 .	39·5	275	9 55 39·7	9 55 40·0
1903 October 23 ...	33·8	485	9 55 41·5	9 55 39·7
1904 October 14 ...	26·0	415	9 55 41·6	
Mean ...	...	....	9 55 41·4	9 55 40·5

The dates in the first column are not the dates of opposition, but dates arbitrarily selected, corresponding generally nearly to the middle of the series of observations, but this would only very slightly affect the resulting figures. It will be seen that there is a similar difference to that pointed out by Mr. Phillips as affecting his own observations, but of larger amount, the deduced rotation periods exceeding the intermediate ones by $0^{\mathrm{s}}\cdot 9$ on the average. It should be remarked, however, that the first two or three results in column 4 above are somewhat uncertain, owing to the small number of the observations, the faintness of the spot, and the very low altitude of the planet. Also that the motion of the spot in 1904 was not uniform, and a period nearly a second shorter than that here given might have been deduced if the first two or three observations had been used. Having regard to these circumstances there seems to be an element of doubt as to the reality, and above all as to the magnitude, of any systematic error here, particularly since any such systematic error is entangled with the real variations that have occurred in the motion of the spot during the last few years*.

One thing seems to be clear, however. Since my observations were made with the eyes parallel to the belts, the systematic error in question, if it really exists (and as to this I feel very doubtful at present), can hardly be connected with the inclination of the belts to a line through the eyes of the observer. There is, however, a possible cause which might perhaps give rise to differences such as Mr. Phillips has drawn attention to, and which has not hitherto, I think, been taken into consideration. This is the influence possibly produced by phase, or rather phase-darkening, on the observations of a very long marking like the Red Spot or Hollow. At quadrature preceding opposition the disc for some distance from the preceding limb is perceptibly less bright than it it is near the following limb; whilst at quadrature following opposition the contrary is the case. The spot and Hollow are so long that when in mid-transit they extend over nearly a third of a chord; and consequently in practice the observation of a transit consists in comparing the comparatively small space between one end and the adjacent limb with the space between the other end and adjacent limb. Irradiation might make the brighter space appear slightly larger than the fainter one, and this would cause an observer to observe the transits a little too early before, and a little too late after opposition.

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* I believe that all observers are agreed as to the motion of the spot or Hollow having been irregular during the last few years. The latter part of a diagram published in 'Popular Astronomy,' 1905, p. 27, by Prof. G. W. Hough, shows the large and comparatively sudden nature of the changes in the motion of the spot as indicated by his micrometer measures.

In conclusion, it would be interesting if other observers can be induced to give the results of their experience, and it is to be hoped also that Prof. Hough will shortly publish his micrometer observations of the Red Spot made during the last few years, as these would be particularly desirable just now, not only for comparison with the eye-estimated observations, but also in connection with the recent real variations in the motion of the spot. As many spots have been observed here during two or more consecutive oppositions, I hope before long to be able to go into the matter of this possibly systematic error more fully.

20 Hove Park Villas, Hove,
1905, Sept. 13.

Yours faithfully,
A. Stanley Williams.

Honoré Flaugergues.

Gentlemen,—

Very little is known of this astronomer, but as he was the first discoverer of the greatest comet of the nineteenth century, something about him may be of general interest. The way in which his life spanned the most momentous period in the political history of his country is almost similar to that of Sir Isaac Newton. The latter was born in the year when the civil war commenced in the reign of Charles I., and died the same year as George I. Flaugergues was born in the year of the accession of Louis XVI., lived through the period of the revolution, the empire, the restoration to the end of the direct line, and died during the reign of the citizen king, Louis Philippe.

His birthplace was Viviers, on the Rhone, in what became the department Ardèche. "On rapporte," we are told in the 'Grand Dictionnaire' of Larousse, "que ce savant n'est jamais sorti de sa ville natale, où il fut juge de paix. Pour ne pas la quitter, il refusa la place de directeur de l'Observatoire de Toulon en 1797."

In the preceding year he had been elected an Associate of the Institut National, to the memoirs of which he had already made several contributions, particularly one on the place of the node of Saturn's ring in 1790, and in the 'Recueil de l'Institut' appeared also the results of a number of miscellaneous astronomical observations made at Viviers between that year and 1798. His earliest publication was, however, one "Sur la théorie des Machines simples," in 1799, and many subsequent papers of great merit appeared from time to time, chiefly, but not wholly, astronomical. Most of these were published in the *Journal de Physique*. It was on the 26th of March, 1811 *, that he discovered the famous comet of that year, which continued in view until the summer of 1812. Cometary observations had indeed a special attraction for him, and in 1815 he obtained a prize offered by the Academy of Nîmes on the question, "Soumettre à une discussion soigneuse toutes les diverses hypothèses imaginées jusque ici pour expliquer l'apparence connue sous le nom de queue, chevelure ou barbe des comètes."

Observations of Mars appeared in 1813, and several papers were published on the heat of the Sun's rays, particularly during the annular eclipse of the 7th of September, 1820. I have not been able to find the reference to his observations of the aureole surrounding the planet Mercury in 1789, which is mentioned by Miss Clerke in her 'History' (4th edition, p. 244). His last papers, on the action of the Moon in diminishing the pressure of the atmosphere, appeared in 1827 and 1829. The varied nature of his scientific studies is illustrated by his obtaining prizes from the Academies of Lyons, Montpellier, and Toulouse for papers on the different refrangibility of the rays of light and on the figure of the Earth, on the rainbow, and on waterspouts respectively. Lalande describes him as "un de nos astronomes les plus utiles." He died at Viviers on the 26th of November, 1835, in the eighty-first year of his age.

Yours faithfully,

Blackheath, 1905, Sept. 15. W. T. LYNN.

NOTES.

COMET NOTES.—*Ast. Nach.* 4047 contains definitive elements of Comet 1886 VIII. by Erik Fagerholm, of Upsala:—

T........	1886 Nov. 28·4428	±0·0027	Berlin M.T.
ω........	31° 55′ 35″	±18″	1887·0.
☊	258 13 1	± 4	
i........	85 35 17	± 4	
log q	0·170471	±0·000021	

Epoch of osculation 1887 April 12.

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* After his observations, the next were obtained at Marseilles on the 11th of April.

There is no decided evidence of departure from a parabolic orbit.
A. C. D. C.

MINOR PLANET NOTES.—The following planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Magnitude.
QX	1905 July 30	Wolf.	12·3
QZ	Sept. 4	Götz.	12·3
RA	18	,,	12·0
RB	19	,,	12·0
RC	Details not yet received.		
RD			
RE			
RF	1905 Sept. 22	Wolf.	11·6

A planet named QY proved not to be new, but identical with 167 Urda. It showed decided variations in brightness.

The planet QV announced last month is probably identical with 263 Dresda, while the body at first supposed to be Dresda is probably new and is the QX given above.
A. C. D. C.

JUPITER'S NEW SATELLITES. — Dr. Ross has published the following revised elements of VI., using the observations of the satellite made in July:—

Epoch	1905 Jan. 0·0 G.M.T.
Mean Jovicentric R.A.	289°·1
R.A. of Perijove	270
R.A. of N. Pole of orbit	86 ·7
Declination	84 ·52
Semi-axis major at Jupiter's Mean Dist.	50′·6
Period	251 days.
Eccentricity	0·156

Ephemeris for Greenwich Noon.

	Pos.-angle.	Distance.		Pos.-angle.	Distance.
Oct. 4	268°	55′	Nov. 3	240°	44′
9	264	55	8	233	40
14	260	55	13	225	36
19	256	53	18	214	33
24	251	51	23	201	30
29	246	48	28	186	29

Several photographs of the satellite were obtained at Greenwich in August and September; the places are in good accord with the ephemeris.

The following positions of VII. were obtained by Mr. Albrecht at the Lick Observatory:—

G.M.T.	Pos-angle.	Distance.
Aug. 7·96	289°·7	54′·6
8·96	289 ·5	55 ·1
9·96	289 ·4	55 ·6

These appear to establish the fact that the motion is Direct, not Retrograde. A graphical method indicates a period of 256 days, eccentricity 0·22, longitude of perijove 132°, semi-axis major at distance 5·2 51′·4, the position of orbit-plane approximately the same as in Ross's elements. A. C. D. C.

Obituary.—Rear-Admiral Sir W. Wharton. A telegram was received on September 29th from Reuter's Cape Town correspondent announcing that Rear-Admiral Sir William James Lloyd Wharton, K.C.B., F.R.S., &c., had died in South Africa at the age of sixty-two. He was one of the members of the British Association who went out to South Africa, and, being taken ill with enteric fever, was not able to undertake the return journey with the remainder of the party. The deceased Admiral was the second son of the late Mr. Robert Wharton, County-Court Judge of York, was born in London on March 2nd, 1843, and was educated privately at Woodcote, and at the Royal Naval Academy, Gosport. He was entered for the Naval Service in August 1857, and won the Beaufort testimonial in 1865. Reaching the rank of Commander in March 1872, while in command of the 'Sheerwater,' he performed most meritorious services in the surveying service in the Dardanelles, the Bosphorus, the Gulf of Zeros, Sea of Marmora, and other waters. During his conduct of these surveys he contributed a most valuable addition to science in settling the many controversies respecting the exhaustless flow of water through the Bosphorus. He proved that an under current existed as strong as that on the surface, and that it invariably flowed in exactly an opposite direction. While in command of the 'Fawn' he was employed in surveying services on the East Coast of Africa, and also in an exhaustive survey of the Sea of Marmora; but perhaps his chief work of this kind was the determination of longitudes when in charge of H.M.S. 'Sylvia.' Promoted to be Captain in January 1880, he was appointed Hydrographer of the Admiralty on August 1st, 1884, and held that position for twenty years, only retiring last year. In August 1891 he was placed on the Captain's retired list, and on New Year's Day, 1895, was promoted to the rank of Rear-Admiral. On the celebration of her late Majesty's birthday in the last-named year, he was decorated with the C.B. (Civil Division), and at the Diamond Jubilee was promoted to be a K.C.B. Sir William Wharton was a Fellow of the Royal Society and of the Royal Geographical and Astronomical Societies, and had been a member of the Joint Antarctic Committee of the Royal and Royal Geographical Societies. He was the author of 'The History of H.M.S. Victory' and of 'Hydrographical Surveying,' a description of the means and methods employed in constructing marine chains, and edited 'Captain Cook's Journal during the First Voyage Round the World, 1768–71,' a literal transcription of the original manuscript with notes and introduction. In his official connection with Greenwich Observatory he was much esteemed by the members of the staff.

The British Association.—Astronomy formed a large feature of the meetings in South Africa. It is hoped to give extracts from the address of Prof. Forsyth, President of the Mathematical and Physical Section, who referred extensively to the work of Edmond Halley, in a subsequent number, and also some account of the addresses of Lord Rosse, Mr. Hinks, and Dr. Alex. Roberts.

We are informed that the opportunity was taken of the presence of Lord Selborne among so many astronomers to lay before His Excellency reasons why an astronomical observatory should be established at Johannesburg. A meeting was arranged at the existing meteorological observatory, at which Lord Rosse, Sir D. Gill, Dr. Backlund, Prof. Kapteyn, Dr. Rambaut, and Mr. Hinks were present. Lord Selborne could not promise the funds, but in his speech at the Meeting of the Association at Johannesburg on August 30 he stated the case and urged some millionaire to come forward and put down the cash.

The eclipse did not pass unnoticed in the daily papers nor in the comic journals. *Punch* made several references, and there is some humour in this incident recorded in a local paper:—

> The headmaster of a school received from a father of two pupils the following letter accounting for their absence:—"Dear Sir, please excuse William and John from school yesterday (Wednesday). I was afraid to send them because of the eclipse."

It does not appear to be generally known that the American astronomer Mr. Percival Lowell, well known from his work on Mars, has been for some years Professor of Astronomy in the Massachusetts Institute of Technology. His correspondents may like to know of this.

There was a slight slip in the last note in our September number. It is the Council of the Meteorological Society which is organizing the lectures, and not the Meteorological Council, which is a rather different body.

Apparently through an accident in the post the copy of some Notes "from an Oxford Note-Book," posted at Cairo in August, did not reach the Editors, and the September Number had to be published without this usual feature. The Notes which follow are the second section of a series relating to an expedition to Egypt to observe the solar eclipse. If the first section is recovered it will be printed later.

From an Oxford Note-Book.

Savoy Hotel, Aswan, Aug. 10-21.

The experiences of the last few days suggest that there may be worse places for an eclipse-station than a big hotel out of season.

There are considerable resources at command and they are freely put at our disposal. There are, of course, many empty rooms at this time of year (for the thermometer keeps above 100° most of the day), and we reap the benefit in having them for store-rooms and dark-rooms in plenty: the hotel boats (for we are on an island in mid-stream) seem but to exist in order to bring instruments and appliances to and fro; and the grounds are parcelled out in sites for the three expeditions—English, American, and Russian—which have their headquarters here. Leave was even given us to plant our instruments on the croquet-lawn, but when we found the preparations were causing anxiety we moved elsewhere.

This morning (August 11) we all took a friendly interest in selecting a site of a rather different kind. Dr. Doubinsky, of Pawlowsk, wishes to set up a self-registering magnetometer to obtain records during the eclipse, and we climbed up to the tombs on the west river-bank (away from the town of Aswan with its railway), and found one which he declared would suit admirably. The only inscription on the walls is deep-sunk in a recess where he could not possibly damage it if he wished; there is plenty of room ; the floor, hewn out of the solid rock, offers the steadiest of foundations for his instruments; and, finally, the whole can be rendered completely dark for photography in a few minutes by covering the doorway with a sheet or two of brown paper. If the old Sun-worshipper whose tomb it is could be asked, he surely would not refuse permission to plant in this place an instrument which may possibly tell us some little thing about the Sun, whatever might be his opinion of the ordinary tourist. And, indeed, the locality seems so appropriate that one quite envies Dr. Doubinsky his opportunities, in spite of the daily stiff climb to his work which they involve. As we mounted the steps this morning we felt glad that we were not dragging up the incline a huge block for the building of one of the tombs, as the builders did some thousands of years ago: peace to their weary limbs!

We are at present four Englishmen, three Americans (one of them a lady), and three Russians. We make excursions into various languages, but were at a loss yesterday morning when the contractor came to arrange about building our piers, and declared he could only talk Greek. How truly wise are those who insist upon a classical education at all costs! Two of the Englishmen are kindly lent by the Survey Department to talk Arabic for us (and to do a great deal of other work incidentally), though most of us have learnt the Arabic for water and ice, and practice the use of these words constantly. We have other drinks, but these can usually be obtained by mentioning their English names—which is convenient, since the high temperature and very dry air cause considerable " transpiration."

Among special drinks to which we have been introduced is the famous Soudan drink called Abu Hammed. On this whole subject a famous chapter was written by the late G. W. Steevens in 'With Kitchener to Khartoum,' and it would be superfluous to add anything but cordial appreciation to what he has said: except, perhaps, an observation on the native drink, of which, if my memory serves me, he said little. The native drink is Nile water, collected in any convenient receptacle and unfiltered. The banks are continually visited by people of all ages and sexes with kerosene tins or other vessels, which they carry off full of water. In fact the Nile has been described as a "muddy ditch, with the whole of the population trying to bale it dry." For there is a considerable quantity of mud freely mixed with this water, so that as a beverage it is, and has, an acquired taste. But once acquired the craving for mud is apparently like any other fleshly desire—it increases with practice. This assertion is prompted by the discovery that the water in a disused bath, in which a few thousand bricks have been soaked for building our piers, is preferred for drinking even to the unadulterated Nile itself.

There is a native village on the island in addition to the hotel. We hear them at night making native music, which is most attractive at a distance, especially the yodel of the women, which we are told is a sign of applause. We also at times hear them or their fellows by day—when they are launching a boat, for instance, which is done with great shouting and encouragement. This simple folk make as much noise about an operation of that kind as we should about a football match. I wonder how they will express their views about the eclipse? Down by the river, too, we see a good many of them, collecting water or fishing. Fishing takes a number of forms. The man of capital has one of the circular nets we have heard of, to cast flat on the top of the water, there being lead weights all round the rim, which fall together and convert the net into a balloon or birdcage, with the fish inside. If your capital is limited you may bring out an old sack and get someone to drive the fish into it. Or, if you have not even a sack, a good deal may be done by sitting in the water near the edge and groping in the mud with the fingers for the fish, which when caught are strung on a string held in the teeth. This last looks a delightful occupation, and we have tried it while bathing, but find we have not sufficient skill. Nevertheless, there is enjoyment to be got from the mere trying, far more than from sitting in a punt on the Thames; for to be in the warm Nile waters under the blue vault of heaven when the Sun is low is a thing to live for.

The Sun is low twice a day, morning and evening; and both times we bathe in the Nile. It is easy to get up here at sunrise, for if the Sun does not waken you, a fly will. It does not require more than one fly, and he can not only wake you but keep you awake, so that it is soon found to be the simplest plan to get up and have an early swim; and then see how the piers and huts are getting on. Indeed it is not difficult to wake earlier still; and on several mornings we have seen the "false dawn," and find that the facts are pretty much as follows:—There is a pale brightening on the horizon at least an hour and a quarter before sunrise, which gradually spreads upwards until the horizon is not sensibly brighter than the sky above; then the horizon again brightens with a brick-red flush, which also mounts upwards, meantime losing its colour; and following this comes the sunrise. Is not the "false dawn" therefore simply the beginning of twilight? The phenomenon is apparently well known here—the Mudir described it accurately and assigned the same * ceremonial function to it. Curiously enough he was of opinion that the "after-glow" in the evening was a mere optical illusion!

WHILE considering the phenomena of dawn, I will set down the following curious passage from Stevenson's delightful 'Travels with a Donkey,' which I found in the hotel library and was glad to read once more. He was awakened by a dog on October 2:—

> The stars were not yet quite extinguished. The heaven was of that enchanting mild gray-blue of the early morn. A still clear light began to fall, and the trees on the hillside were outlined sharply against the sky. The wind had veered more to the north, and no longer reached me in the glen; but as I was going on with my preparations, it drove a white cloud very swiftly over the hill-top; and looking up, I was surprised to see the cloud dyed with gold. In these high regions of the air the Sun was already shining as at noon. If only the clouds travelled high enough, we should see the same thing all night long. For it is always daylight in the fields of space.

The error is merely one of dimensions. The gifted author has made the atmosphere very much too high and the Earth's shadow very much too low; to put him right his clouds would have to be far beyond the Moon, and the Moon cannot be regarded as always being in the "fields of space" since she suffers eclipse. It is perhaps less curious that when Stevenson does venture on a scientific statement he should be unfortunate than that he should care to make one at all. Only a page earlier he appears to waive aside all such methods of regarding Nature:

> No one knows the stars who has not slept, as the French happily put it, *à la belle étoile.* He may know all their names and distances and magnitudes, and yet be ignorant of what alone concerns mankind, their serene and gladsome influence on the mind. The greater part of poetry is about the stars; and very justly, for they are themselves the most classical of poets.

It is worth remembering that this is the son of the man who first exposed thermometers in a "Stevenson screen"; and that the poetic son was proud of his prosaic father.

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* The reference is to a paragraph in the Notes which have gone astray in the post, and which it is hoped to recover.

But I fear I have not yet said much about the eclipse preparations. Perhaps there is not much to say: the building of piers and shelters from wind and Sun and the setting up of instruments —all this means pretty hard and constant work, but there is not much to say about it. The days pass serenely, perhaps because they are so fully occupied that we scarcely notice how busy we are. A small incident revealed to us something of the continuity of the work yesterday. Two of us went across to welcome the return of Captain Lyons, and finding the train signalled 1½ hours late, we automatically returned to our island to continue operations, in the meantime. But on again reaching the station we were told that there was still half-an-hour. To cross the Nile twice again would take up the whole of that time and was clearly out of the question; so we strolled along the bank to a café, and sat quietly down in the main street of Aswan and had a contemplative glass of beer; and only when the pleasure of the inactivity gradually forced itself on our attention did we realize how few such moments there had been in the last ten days or more. We had a splendid view of the western horizon over the Nile; on August 30 others will sit where we sat and watch the eclipse framed in a truly gorgeous setting. Think of it—to sit quietly down in the street outside your own shop door, and have a total eclipse spread out right in front of you! But will any do this after all? Will they not rather climb on to their roof to get a better view? We shall not be there to see.

There are, however, some incidents in our day. There is, for instance, the despatch of telegrams to Cairo for things forgotten, which the omni-capable Survey Department promptly sends us. There is the receipt of telegrams about things or persons on the way to us. Sometimes these get a little altered in the sending and make amusing reading; so that it has become the practice to put up all telegrams on the hotel notice-board for the general good. (We practically have the run of the hotel.) And we also get the *Times* very regularly, or, as our American friends call it, the *London Times*. On the notice-board, too, there appear occasional anonymous contributions, a specimen of which I am tempted to subjoin. But I must first explain one or two allusions.

The American party have with them a composite bundle of four telescopes, all pointing in different directions, with which they intend to sweep the neighbourhood of the eclipsed Sun for intra-Mercurial planets. The divergence in direction has secured for this instrument the name "Cross-eyed Lady." Further, this same party has determined the latitude of their station (which is in sight of three reference-points of the Survey) with a sextant! I leave it to you to imagine the wrath and scorn of the Survey

Department, and to judge who is likely to be the real author of the following document (for the *nom de plume* see above):—

NOTE ON THE DETERMINATION OF THE LATITUDE OF THE CROSS-EYED LADY.

By ABU HAMMED.

With a common sextant observations of Polaris were made after dinner. Some uncertainty arose as to which of two stars about 1 degree apart was really the star under observation, so the mean was taken. The measured altitude of this mean star was 24° O. (the sextant being divided in degrees). The observer's watch (price one dollar) had stopped, but the time elapsed since drinks indicated a time of about 9 o'clock P.M. Consequently we must add to the above a correction of 5′ 24″·330. The horizon used was between one kilometre and two kilometres distant, the height of the observer 76·300′ R.S.L., and the Aswan dam reading 74·827 R.S.L.; consequently we have the further correction of $19''\cdot\dot{9}9\dot{9}$. In addition to the above we must not omit to apply the correction for lateral refraction (±0″·000) and for errors of sextant divisions (close on 0′·000).

The final result obtained was

24° 5′ 43″·007,

confirming in a remarkable way the Survey Department's value, viz. 24° 5′ 34″, which is in error by 1′ in defect.

ONE further allusion I see requires a footnote. "Lateral refraction" has been one of the topics of conversation, and as an outcome of our discussions I will offer to those interested the following two problems for consideration:—

(*a*) Suppose an absolutely straight river, over which hangs a straight plate of atmosphere with an unknown index of refraction. A survey of the neighbourhood is made, measuring base lines as often as you like on both sides of the river, but not, of course, across the river: points on opposite sides of the river are only connected by theodolite observations. Is it possible to determine the amount of "lateral refraction"?

(*b*) Is the possibility altered if there are curvatures in the river?

AT the time of writing we have had a great scare. The afternoon of August 21 has been quite cloudy, so that if this had been eclipse-day we should have seen nothing at all. So much for "a certainty." We begin to feel anxious about the future, and have consulted the 'Coptic Calendar' about eclipse-week. The entries are as follows:—

August	24.	The Sun in Virgo.
	25.	Itching of the body.
	26.	Festival of the Companions of the Cave (the Seven Sleepers). Sleep becomes heavier.
	27.	Winds very variable.
	28.	Abundance of mosquitoes.
	29.	The taste of fruit is affected by the Nile water.
	30.	Avoid female society. End of sowing autumn dūrah.
	31.	Sour milk should be taken. Going to the bath is disliked.
Sept.	1.	Sowing of turnip and beetroot.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXVIII. NOVEMBER, 1905. No. 363.

Meeting of the British Association in South Africa, 1905.— Address by the President, Prof. G. H. Darwin, M.A., F.R.S.

Cosmical Evolution.

[Concluded from p. 370.]

Only a few thousand stars are visible with the unaided eye, but photography has revealed an inconceivably vast multitude of stars and nebulæ, and every improvement in that art seems to disclose yet more and more. About twenty years ago the number of photographic objects in the heavens was roughly estimated at about 170 millions, and some ten years later it had increased to about 400 millions. Although Newcomb, in his recent book on 'The Stars,' refrains even from conjecturing any definite number, yet I suppose that the enormous number of 400 million must now be far below the mark, and photography still grows better year by year. It seems useless to consider whether the number of stars has any limit, for infinite number, space, and time transcend our powers of comprehension. We must then make a virtue of necessity, and confine our attention to such more limited views as seem within our powers.

A celestial photograph looks at first like a dark sheet of paper splashed with whitewash, but further examination shows that there is some degree of method in the arrangement of the white spots. It may be observed that the stars in many places are arranged in lines and sweeping trains, and chains of stars, arranged in roughly parallel curves, seem to be drawn round some centre. A surface splashed at hazard might present apparent evidence of system in a few instances, but the frequency of the occurrence in the heavens renders the hypothesis of mere chance altogether incredible.

Thus there is order of some sort in the heavens, and, although no reason can be assigned for the observed arrangement in any particular case, yet it is possible to obtain general ideas as to the succession of events in stellar evolution.

Besides the stars there are numerous streaks, wisps, and agglomerations of nebulosity, whose light we know to emanate from gas. Spots of intenser light are observed in less brilliant regions; clusters of stars are sometimes imbedded in nebulosity, while in other cases each individual star of a cluster stands out clear by itself. These and other observations force on us the conviction that the wispy clouds represent the earliest stage of development, the more condensed nebulæ a later stage, and the stars themselves the last stage. This view is in agreement with the nebular hypothesis of Laplace, and we may fairly conjecture that the chains and lines of stars represent pre-existing streaks of nebulosity.

As a star cools it must change, and the changes which it undergoes constitute its life-history, hence the history of a star presents an analogy with the life of an individual animal. Now, the object which I have had in view has been to trace types or species in the physical world through their transformations into other types. Accordingly it falls somewhat outside the scope of this address to consider the constitution and history of an individual star, interesting although those questions are. I may, however, mention that the constitution of gaseous stars was first discussed from the theoretical side by Lane, and subsequently more completely by Ritter. On the observational side the spectroscope has proved to be a powerful instrument in analysing the constitutions of the stars, and in assigning to them their respective stages of development.

If we are correct in believing that stars are condensations of matter originally more widely spread, a certain space surrounding each star must have been cleared of nebulosity in the course of its formation. Much thought has been devoted to the determination of the distribution of the stars in space, and although the results are lacking in precision, yet it has been found possible to arrive at a rough determination of the average distance from star to star. It has been concluded, from investigations into which I cannot enter, that if we draw a sphere round the Sun with a radius of twenty million millions of miles*, it will contain no other star; if the radius were twice as great the sphere might perhaps contain one other star; a sphere with a radius of sixty million millions of miles will contain about four stars. This serves to give some idea of the extraordinary sparseness of the average stellar population; but there are probably in the heavens urban and rural districts, as on Earth, where the stars may be either more or less crowded. The stars are moving relatively to one another with speeds which are enormous, as estimated by terrestrial standards, but the distances which separate us from them are so immense that it needs refined observation to detect and measure the movements.

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* This is the distance at which the Earth's distance from the Sun would appear to be 1″.

Change is obviously in progress everywhere, as well in each individual nebula and star as in the positions of these bodies relatively to one another. But we are unable even to form conjectures as to the tendency of the evolution which is going on. This being so, we cannot expect, by considering the distribution of stars and nebulæ, to find many illustrations of the general laws of evolution which I have attempted to explain; accordingly I must confine myself to the few cases where we at least fancy ourselves able to form ideas as to the stages by which the present conditions have been reached.

Up to a few years ago there was no evidence that the law of gravitation extended to the stars, and even now there is nothing to prove the transmission of gravity from star to star. But in the neighbourhood of many stars the existence of gravity is now as clearly demonstrated as within the solar system itself. The telescope has disclosed the double character of a large number of stars, and the relative motions of the pairs of companions have been observed with the same assiduity as that of the planets. When the relative orbit of a pair of binary or double stars is examined, it is found that the motion conforms exactly to those laws of Kepler which prove that the planets circle round the Sun under the action of solar gravitation. The success of the hypothesis of stellar gravitation has been so complete that astronomers have not hesitated to explain the anomalous motion of a seemingly single star by the existence of a dark companion; and it is interesting to know that the more powerful telescopes of recent times have disclosed, in at least two cases, a faintly luminous companion in the position which had been assigned to it by theory.

By an extension of the same argument, certain variations in the spectra of a considerable number of stars have been pronounced to prove them each to be really double, although in general the pair may be so distant that they will probably always remain single to our sight. Lastly, the variability in the light of other apparently single stars has proved them to be really double. A pair of stars may partially or wholly cover one another as they revolve in their orbit, and the light of the seemingly single star will then be eclipsed, just as a lighthouse winks when the light is periodically hidden by a revolving shutter. Exact measurements of the character of the variability in the light have rendered it possible not only to determine the nature of the orbit described, but even to discover the figures and densities of the two components which are fused together by the enormous distance of our point of view. This is a branch of astronomy to which much careful observation and skilful analysis has been devoted; and I am glad to mention that Alexander Roberts, one of the most eminent of the astronomers who have considered the nature of variable stars, is a resident in South Africa.

I must not, however, allow you to suppose that the theory of eclipses will serve to explain the variability of all stars, for there are undoubtedly others whose periodicity must be explained by something in their internal constitution.

The periods of double stars are extremely various, and naturally those of short period have been the first noted; in times to come others with longer and longer periods will certainly be discovered. A leading characteristic of all these double stars is that the two companions do not differ enormously in mass from one another. In this respect these systems present a strongly marked contrast with that of the Sun, attended as it is by relatively insignificant planets.

In the earlier part of my address I showed how theory indicates that a rotating fluid body will as it cools separate into two detached masses. Mathematicians have not yet been able to carry their analysis far enough to determine the relative magnitudes of the two parts, but as far as we can see the results point to the birth of a satellite whose mass is a considerable fraction of that of its parent. Accordingly See (who devotes his attention largely to the astronomy of double stars), Roberts, and others consider that what they have observed in the heavens is in agreement with the indications of theory. It thus appears that there is reason to hold that double stars have been generated by the division of primitive and more diffused single stars.

But if this theory is correct we should expect the orbit of a double star to be approximately circular; yet this is so far from being the case that the eccentricity of the orbits of many double stars exceeds by far any of the eccentricities in the solar system. Now See has pointed out that when two bodies of not very unequal masses revolve round one another in close proximity the conditions are such as to make tidal friction as efficient as possible in transforming the orbit. Hence we seem to see in tidal friction a cause which may have sufficed not only to separate the two component stars from one another, but also to render the orbit eccentric.

I have thought it best to deal very briefly with stellar astronomy, in spite of the importance of the subject, because the direction of the changes in progress is in general too vague to admit of the formation of profitable theories.

We have seen that it is possible to trace the solar system back to a primitive nebula with some degree of confidence, and that there is reason to believe that the stars in general have originated in the same manner. But such primitive nebulæ stand in as much need of explanation as their stellar offspring. Thus, even if we grant the exact truth of these theories, the advance towards an explanation of the universe remains miserably slight. Man is but a microscopic being relatively to astronomical space, and he lives on a puny planet circling round a star of inferior rank. Does it not then seem as futile to imagine that he can discover the origin and tendency of the universe as to expect a housefly to instruct us as to the theory of the motions of the planets? And yet, so long as he shall last, he will pursue his search, and will no doubt discover many wonderful things which are still hidden. We may indeed be amazed at all that man has been able to find out, but the immeasuable magnitude of the undiscovered will throughout all time remain to humble his pride. Our children's children will still be gazing and marvelling at the starry heavens, but the riddle will never be read.

EXTRACTS FROM ADDRESSES.

An Appreciation of Halley.

By Prof. A. R. Forsyth, Sc.D., F.R.S.

Having thus noted some part of the stir in scientific life which marked the late years of the seventeenth century, let me pass to the second of our centenaries: it belongs to the name of Edmond Halley. Quite independently of his achievement connected with the year 1705, to which I am about to refer, there are special reasons for honouring Halley's name in this Section at our Meeting in South Africa. When a young man of twenty-one he left England for St. Helena, and there, in the years 1676–1678, he laid the foundations of stellar astronomy for the Southern Hemisphere; moreover, in the course of his work he there succeeded in securing the first complete observation of a transit of Mercury. After his return to England, the next few years of his life were spent in laying science under a special debt that can hardly be over-appreciated. He placed himself in personal relation with Newton, propounded to him questions and offered information; and it is now a commonplace statement that Halley's questions and suggestions caused Newton to write the 'Principia.' More than this, we know that Newton's great treatise saw the light only through Halley's persuasive insistence, through his unwearying diligence in saving Newton all cares and trouble and even pecuniary expense, and through his absolutely self-sacrificing devotion to what he made an unwavering duty at that epoch in his life. Again, he appears to have been the first organiser of a scientific expedition, as distinct from a journey of discovery, towards the Southern Seas: he sailed as far as the fifty-second degree of southern latitude, devised the principle of the sextant in the course of his voyaging, and, as a result of the voyage, he produced a General Chart of the Atlantic Ocean, with special reference to the deviation of the compass. Original, touched with genius, cheery of soul, strenuous in thought, and generous by nature, he spent his life in a continuously productive devotion to astronomical science, from boyhood to a span of years far beyond that which satisfied the Psalmist's broodings. I have selected a characteristic incident in his scientific activity, one of the most brilliant (though it cannot be claimed as the most important) of his astronomical achievements; it strikes me as one of the most chivalrously bold acts of convinced science within my knowledge. It is only the story of a comet.

I have just explained, very briefly, Halley's share in the production of Newton's 'Principia'; his close concern with it made him the Mahomet of the new dispensation of the astronomical universe, and he was prepared to view all its phenomena in the light of that dispensation. A comet had appeared in 1682—it was still the age when scientific men could think that, by a collision between the Earth and a comet, "this most beautiful order of things would be entirely destroyed and reduced to its ancient chaos"; but this fear was taken as a "by-the-bye," which happily interfered with neither observations nor calculations. Observations had duly been made. The data were used to obtain the elements of the orbit, employing Newton's theory as a working hypothesis; and he expresses an incidental regret as to the intrinsic errors of assumed numerical elements and of recorded observations. It then occurred to Halley to calculate similarly the elements of the comet which Kepler and others had seen in 1607, and of which records had been made; the Newtonian theory gave elements in close accord with those belonging to the comet calculated from the latest observations, though a new regret is expressed that the 1607 observations had not been made with more accuracy. On these results he committed himself (being then a man of forty-nine years of age) to a prophecy (which could not be checked for fifty-three years to come) that the comet would return about the end of the year 1758 or the beginning of the next succeeding year; he was willing to leave his conclusion "to be discussed by the care of posterity, after the truth is found out by the event." But not completely content with this stage of his work, he obtained with difficulty a book by Apian, giving an account of a comet seen in 1531, and recording a number of observations. Halley, constant to his faith in the Newtonian hypothesis, used that hypothesis to calculate the elements of the orbit of the Apian comet; once more regretting the uncertainty of the data and discounting a very grievous error committed by Apian himself, Halley concluded that the Apian comet of 1531, and the Kepler comet of 1607, and the observed comet of 1682 were one and the same. He confirmed his prediction as to the date of its return, and he concludes his argument with a blend of confidence and patriotism :—

> Wherefore if according to what we have already said it should return again about the year 1758, candid posterity will not refuse to acknowledge that this was first discovered by an *Englishman.*

Such was Halley's prediction published in the year 1705. The comet pursued its course, and it was next seen on Christmas Day 1758. Candid posterity, so far from refusing to acknowledge that the discovery was made by an Englishman, has linked Halley's name with the comet, possibly for all time.

We all now could make announcements on the subject of Halley's comet; their fulfilment could be awaited serenely. No vision or inspiration is needed; calculations and corrections will suffice. The comet was seen in 1835, and it is expected again in 1910. No doubt our astronomers will be ready for it, and the added knowledge of electrical science, in connection particularly with the properties of matter, may enable them to review Bessel's often-discussed conjecture as to an explanation of the emission of a sunward tail. But Halley's announcement was made during what may be called the immaturity of the gravitation theory; the realization of the prediction did much to strengthen the belief in the theory and to spread its general acceptance; the crown of conviction was attained with the work of Adams and Leverrier in the discovery, propounded by theory and verified by observation, of the planet Neptune. I do not know an apter illustration of Bacon's dictum that has already been quoted, "All true and fruitful natural philosophy hath a double scale, ascending from experiments to the invention of causes, and descending from causes to the invention of new experiments." The double process, when it can be carried out, is one of the most effective agents for the increase of trustworthy knowledge. But until the event justified Halley's prediction, the Cartesian vortex-theory of the universe was not completely replaced by the Newtonian theory; the Cartesian votaries were not at once prepared to obey Halley's jubilant, if stern, injunction to "leave off trifling. . . . with their vortices and their absolute plenum . . . . and give themselves up to the study of truth."

Apioidal Binary Star-systems.

By Alexander W. Roberts, D.Sc.

During the past ten years a notable extension has taken place in the area of variable star research.

The scope of this branch of astronomy is no longer simply the ascertainment of the period and amplitude of variation of such stars as fluctuate regularly in brightness, or even the determination of the form and character of their light-curve. Variable star research goes beyond this into a region of investigation, where it marches with the great subjects of cosmic physics and the evolution of binary systems.

Thus the study of variable stars, or at least the study of a certain type of variable star, includes an inquiry into the conditions of magnitude, movement, and position that underlie such changes in brightness as observation reveals to us.

One of the most important of the recent developments in this direction is that dealing with the orbital elements, dimensions, density, tidal pulsations, and secular changes of that remarkable class of stars which for want of a better name we may call apioidal binary systems.

Practical work on the subject of apioidal or prolate binary systems dates only from the close of last century; but to understand the nature and importance of the discoveries which made it possible even to enter upon a practical consideration of the problem it is necessary to go some way back in the history of variable star research *.

. . . .

If two stars form a binary system there are three ways of arriving at a knowledge of the main facts of figure, motion, mass, and density; but no single method yields a full knowledge of these orbital elements. Each method has its limits, but each is supplementary of the other.

It will make for brevity if we state in order the various values which fix the dimensions, size, and position of any binary system.

1. The period of revolution.
2. The size of the orbit.
3. The eccentricity of the orbit.
4. The position of the orbit in space.
5. The inclination of the orbit.
6. The size and form of the component stars.
7. The mass of the components.
8. The density of the components.
9. The surface brightness of the components.

It is evident that determinations of the value of any of the above elements, made at different dates, will yield evidence of any secular change. Such determinations are of special interest when the eccentricity, distance, figure, and brightness of the component stars are in question.

a. If the two components of a binary system are so far apart that they can be readily discerned in a telescope as two points or even as an elongated point of light, then visual observations, when carefully made and correctly treated, will yield 1, 3, 4, and 5.

This method of inquiry will not furnish any information regarding 2, 6, 7, 8, and 9 unless the distance of the system is known, as in the case of *α Centauri* and *Sirius*, and then forthwith 2 and 7 are also known.

The relation which exists between the visual observations of wide binary systems and the elements of the system have been set forth by many astronomers. The writer has found the formulæ given by Asaph Hall † the most convenient in practice.

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* [Dr. Alex. Roberts made reference to the theoretical researches of Prof. G. H. Darwin on egg-shaped or pear-shaped stars, and to the observations of the apioidal systems β Lyræ, U Pegasi, V Puppis, and RR Centauri.—Eds.]

† *Astronomical Journal*, No. 324, p. 89.

It must be remembered that as yet no close binary system has been separated, or even elongated, by any telescope, however powerful. I am not without hope, however, that one day, and that not remote, this may be be accomplished, especially in the case of vast systems like *V Puppis*.

b. If the system is bright enough to come within spectroscopic range, as in the case of many wide and close binary systems, then this examination yields 1, 2, 3, 4, and 7. Spectroscopic observations yield no clue to 5, 6, 8, and 9.

The equations connecting line of sight observation with the elements of a binary system have been set forth with great fulness by Lehmann-Filhes *. I have found, however, the simpler relations established by Rambaut † quite as convenient in practice.

c. If two stars of a close binary system revolve in a plane so situated that they mutually eclipse each other, it is evident that the regular ebb and flow of light thus brought about will be a measure of the brightness, size, position, and figure of the component stars. If, accordingly, we determine from observation the variation of a close binary system we have the means of ascertaining 1, 3, 5, 6, 8, and 9. The relation existing between these elements and the light curve of an apioidal binary system has been set forth by the writer in a paper recently published in the 'Monthly Notices of the Royal Astronomical Society' ‡.

The value of this method of investigation when available, as against *a* and *b*, is at once evident. We know six elements out of the nine, only 2, 4, and 7 remaining unknown. Indeed, with our present limitations, it is only through the avenue of stellar variation that we can arrive at any knowledge whatever of the size and shape of a star.

Researches on Lunar Heat §.

In the years 1869 and 1870 I made some preliminary experiments with the thermopile, placed in the focus of my 3-foot reflector, which showed that the heat radiated from the lunar surface could not only be detected with certainty, but also measured with more or less accuracy.

This heat was shown to be due to the Sun's rays rather than from the interior of the Moon, as on approaching the period of New Moon the heat declined towards an imperceptible amount, and at other times varied much, as might be expected, with the phase.

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* *Astronomische Nachrichten*, No. 3242.

† 'Monthly Notices R. A. S.' vol. li. p. 316.

‡ *Ibid.* vol. lxiii. p. 527.

§ Read by the Earl of Rosse in Section A, British Association, Cape Town, August 18, 1905.

It was also shown by the large absorption—some 80 to 85 per cent.—by a sheet of glass placed in front of the thermopile, which sheet was found to absorb some 10 per cent. only of the direct solar rays, that this heat was largely absorbed by the lunar surface before it was radiated towards the Earth, in fact that the Moon's surface is much heated for the time.

Some rough comparisons of total solar and lunar radiation were also made, and the bearings of the results on the "albedo" of the Moon as determined by Zöllner was gone into.

In the course of the work various improvements were made in the apparatus. The single thermopile of four elements of Messrs. Elliott was replaced by two such, on to which the Moon's heat was directed alternately, with the object of neutralizing various causes of disturbance, while at the same time doubling the lunar effect. Then, as the two Elliott piles were found to differ considerably in power, two home-made single thermo pairs were substituted. These, however, are difficult for amateur hands to make. They are extremely fragile when the proportions are much reduced for sensibility, and they have never been of such equality of power as might be desired.

The galvanometer used (Thomson's reflecting tripod, made by Messrs. Elliott) is not altogether as satisfactory for the work as might be wished.

Towards the close of the year 1870, Dr. Ralph Copeland, on coming to Birr Castle, took up the work. More extended series of determinations were made; an "extinction curve" for varying altitude, similar to that obtained by Seidel at Munich for the light of the stars, was formed as a preliminary to the better comparison of determinations of lunar heat made on different nights at different altitudes, and, finally, a better phase-curve was obtained.

From a similar phase-curve for heat through a sheet of glass it would appear that the percentage of heat passing through it is greatest as the time of Full Moon is approached, being less towards both the first quarter and the last quarter beyond.

On examining the phase-curve (not "through glass") it was observed that, far from showing a maximum later than the time of Full Moon—as might be expected,—it gave an apparent maximum, whether from accidental errors of observation or from a real difference of average radiation from the two sides of the Moon's disc, a little before Full Moon.

It was now proposed to apply a crucial test for the rate of heating of the Moon's surface under the Sun's rays by making determinations during the progress of an eclipse of the Moon, when the change of the total amount of solar heat falling upon the Moon is immensely more rapid than that due to change of phase.

The lagging of the observed heat behind the calculated light under these conditions, though apparently perceptible, is very small, thus showing that it need not be looked for in the phase-curve *.

The subject was laid aside for some years in favour of work on the nebulæ, and nothing further was done worth remarking on until the total eclipse of October 4, 1884, when Dr. Boeddicker, who had meanwhile taken charge of the observatory, favoured exceptionally by the weather, made a long series of determinations.

The rapid decrease and subsequent increase of the heat was confirmed. The observed heat varied nearly in proportion to the calculated light. There was a slight lagging, however, behind the light, but, for some as yet unexplained reason, the heat did not return to its standard amount till long after the eclipse had ceased, the deficiency being about 12 per cent.

Nothing further was done until the occasion of the total eclipse of January 28, 1888, when the sky was not obscured at all during the whole progress of the eclipse, and the same anomaly of the heat not returning to its standard value, even $1^h\ 40^m$ after the last contact with the penumbra, was observed.

But, in addition, the heat, the measurement of which was begun $1^h\ 5^m$ before the first contact with the penumbra, seemed to be even then slightly decreasing, and, we felt fully satisfied, very *perceptibly* decreased 15 minutes before the first contact with the penumbra. This result would appear interesting, if confirmed, as suggesting a terrestrial atmosphere much more extensive than is generally supposed, which intercepts the Sun's rays of heat long before any part of the Moon has reached the Earth's shadow.

The heat during the period of totality was again imperceptible with our apparatus.

Part II.

The time seems now to have come when it may be considered how far the further pursuit of the heat determinations with the present apparatus and according to the present method is likely to be profitable.

Twenty-one years have now elapsed since the non-return to standard value of the eclipse was first observed, and 16 years since it was again noticed. The occasions were exceptional ones for our climate. No such occasion has since arisen, or is likely to arise in a generation, by the present method. Even if the weather be favourable, about half of the eclipses which occur are invisible, and of the remainder many can be observed during part only of their progress, and, further, you practically always have considerable variations of altitude during the limits of time through which it is desirable to pursue the determinations.

If the questions raised, or likely to be raised, are of sufficient interest, it would seem that for more rapid progress to be made the place of observation, if still a single place, should be transferred to where there is as nearly cloudless a sky as possible, together with one of as uniform transparency as can be found, so that an extinction-curve may be more reliable in the reduction of observations. What about a station in Upper Egypt?

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* It was, however, clearly shown that there is a large and rapid decline of radiant heat, somewhere in proportion to the decline of light. The eclipse observed was the partial one of November 14, 1872.

But what is really also wanted is a selection of *several* suitable stations varying much in longitude, so that an eclipse may be watched under favourable conditions of altitude at one or other station during the whole of its progress and for a sufficient period before and after.

Further, that there should be suitable stations also in the south hemisphere, so that eclipses occurring during the English summer season may be observed under as good conditions as those occurring during our winter season can be observed at northern stations.

Apparatus.

With regard to apparatus, it is obvious that for the sufficient concentration of the Moon's heat an optically perfect arrangement is unnecessary.

At Birr Castle, the 3-foot mirror of 27 feet focus, giving an image of the Moon of 3 inches diameter, was taken as we found it. Two concave mirrors of 3½ inches diameter and 3 inches focal length were placed side by side in the plane of the large mirror to collect the heat, each on to a spot of ⅓ inch diameter, at which a thermo pair of ½ inch face was placed.

The same thing could be effected by means of a parabolic* mirror—say a search-light mirror 2 feet diameter and 3 feet focus—of sufficiently good quality, into the focal plane of which would be placed directly the two thermo pairs, with a saving of the loss by the second reflection. The front surface—not the back surface, as for search-lights—would of course be silvered, having been prepared with this view.

Thus, instead of an efficiency of, say, 0·35, with two metallic reflections, we should have one of 0·90, and the whole arrangement would be most compact and handy. The glass need not be more than, say, ½ inch thick nor optically perfect, as for this purpose small flexures are of no importance. It may be observed, also, that all Dr. Copeland's observations were made with an altazimuth mounting only, controlled by hand, so that a clock-movement is not essential, but rough polar and declination axes with two hand worm-wheel controls would clearly be desirable. The whole apparatus could, however, be carried out and laid in a standard position on, say, a concrete base when about to be used.

From what I can learn, such an arrangement could be provided for £100 (perhaps much less), including thermo couples and galvanometer. A 22-inch silvered-glass mirror would be the equivalent of my 36-inch with two speculum metal reflections.

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* Even a spherical surface would appear sufficient for this purpose if of focal length not less than two apertures.

Another apparatus would be of the siderostat form, the primary concave mirror being of, say, 3 feet aperture and, say, 27 feet focal length, the heat being further concentrated by small concave mirrors as at Birr Castle. This form would admit of the use of Boy's radio-micrometer (modified as to size of receiving-surface to catch all the lower rays), which can be worked only in a vertical position. With the increase of sensitiveness which might be looked for, the whole disposition might be reduced in size and cost proportionately.

Langley's bolometer again, properly made and designed, might perhaps be employed with advantage with either the "search-light" or the "siderostat" form of apparatus, but my own experience with a bolometer, made by an English artist who had not made one before, was not encouraging.

As to reflecting surfaces, our experience in the past at Birr Castle has been with metallic reflectors alone, but recent trials have disposed us to look upon silvered glass as in general superior for all purposes. Through recent improvements the process of silvering glass has been rendered more easy and more certain, and its comparative perishability from the damp of the Irish climate has been easily combatted by means of a slight warmth from electric lamps. Two sets of three 8 c. p. lamps in series, consuming in all ¼ ampere at 200 volts, always in circuit, have preserved one 3-foot mirror practically unimpaired for some months (of course we have no coal-smoke in the atmosphere), and by having several spare flats we think that it may be superfluous to trouble with warming that which is for the time being used in the tube.

So far as I know, no one is at present working on the subject of Lunar Radiant Heat. Professor Langley and Mr. Very with the "bolometer," which they found capable of much greater sensitiveness, worked at Allegheny Observatory. They substituted analysis of the heat by the spectrum of a rock-salt prism for the comparative absorption by glass of the solar and lunar rays, or rather in addition to the latter, with which my measurements were confirmed; but Langley does not agree with my conclusions, considering that by means of the prism alone can the temperature of the lunar surface be arrived at. No phase-curve has been derived by them, except one by Very, based on what I venture to think is quite too small a number of determinations. The same may, I think, also be said of his short work on the radiant heat from different portions of the lunar surface.

For work on eclipses, such stations as in U.S. America, Egypt, and India for the Northern, and South America, Cape of Good Hope, and Australia for the Southern Hemisphere might be suggested, and in all probability all that is to be got out of the subject would be obtained in a comparatively short time and with comparatively little labour.

Mr. Hutchins, in America, has made some experiments on the radiation from various surfaces on the Earth, but it seems doubtful how far they bear upon the lunar question, seeing that the conditions are so different. His work is on surfaces in contact with air at the ordinary pressure. The lunar surface is, on the other hand, in contact with a vacuum. ROSSE.

The Astronomical Society of the Atlantic.

AFTER an exciting and all too brief career, the Astronomical Society of the Atlantic has separated, never, one may fear, to enjoy again all the delightful circumstances of its early life. We left it starting south from Madeira on the day of its birth *. Since that day it has travelled through South Africa, a small component of the great party of the British Association on tour, has spent many pleasant days at the Royal Observatory at the Cape, talked late over the *Schlafdrunk* in the dining-saloon of "Train A," and sat together on Livingstone Island above the Victoria Falls. Thereafter it broke up, because the moiety returning by the East Coast failed to convince the other half that the West Coast route was not the more desirable; and before these notes appear it is not improbable that even the East Coast party may have reached Pulkowa, Helsingfors, and Groningen respectively; the western division will have been in Groningen and Cambridge for three weeks or more. It is high time to do what should not have been left in vain to the leisure of the homeward voyage, and to fulfil the promise made to the Editors of this magazine to chronicle in its pages some of the more personal recollections of our astronomical experiences in South Africa. But we must first complete the outward voyage.

From the first day of its life the Society had before it a serious piece of business. The Senior Member from Groningen had made up his mind that he intended to find out what the Milky Way is, and what relation it bears to the rest of the stars in the sky. What is more, he had prepared a mighty scheme for doing it, and each member's baggage was bulging with the explanatory memorandum. So the Society had a noble start, and the record of its proceedings would suggest that it had modestly taken the whole Universe to be its province. The proceedings, however, are not going to be published for a while. They have gone, perhaps, to swell the second edition of that memorandum. The "protocol" of each Meeting has been duly adopted, but the views of each Member as to the way he would solve the riddle of the universe must go for the present unrecorded.

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* See September number, p. 344.

It may not be amiss, however, to speak of some questions that came under discussion, which have an interest wider even than those of the very wide scheme itself. It may be taken for granted that any large scheme of the kind will depend mainly on photographs taken upon centres distributed regularly over the whole sky. But what principles shall govern the selection of those centres? It might seem at first quite obvious that the centres upon which astrographic catalogue and chart plates are taken are points on which future investigations will naturally be centered in their turn. But there are difficulties in the way of accepting this almost self-evident proposition. We have to consider the relation of these centres to the plane of the Milky Way. They must be as symmetrical as possible in galactic latitude and longitude, but must also lie for choice upon those parallels of declination where the Gesellschaft Zone Catalogues overlap, and where we have Pickering's photometric measures down to $9^{m}.0$. And, finally, there is the important consideration that the limits of right ascension of the plates must overlap as little as possible, or it will hamper the work of meridian observers determining standard stars for proper motions.

One cannot satisfy all these conditions, and it becomes a nice question which should be sacrificed. A photographer professing the heterodox opinion that proper motions can be determined photographically without reference to meridian places would be apt to give too little weight perhaps to the last consideration, and a vivid recollection of the trouble that is involved in reducing Eros plates taken on all sorts of centres adds strength to the conviction that there are substantial advantages in keeping to centres that other people have used. But it is hard to maintain this position against the deliberate opinion of men who have examined all sides of the question and propose to work on different lines, and it seems likely that future large schemes of astrographic work will not always be based upon centres which are already to some extent classical.

There is another question upon which opinions may differ widely. If one is selecting a couple of hundred sample regions upon which to base a general investigation of the structure of the sky, should one include or exclude regions which appear to be in any way peculiar—should one, for example, consider as normal a region in the centre of one of the rich star-clouds of the Milky Way? The question cannot be answered without some assumption as to the nature of the Milky Way structure; therefore it will be necessary to include, but to discuss separately, both the clouds and the rifts, which puts a strain at once upon any orderly scheme of distribution. And how shall the Magellanic Clouds be dealt with? They may contain, as has been suggested, the key to the structure of the whole universe; they may, on the other hand, support the view that the universe is not one but many structures, more or less independent, and obeying different laws of distribution. They may well require a whole complete investigation to themselves, and are at any rate likely to bring violent discontinuities into a general scheme of discussion.

Again, should one go for telescopes of great focal length, or work with those of the "astrographic" dimensions? It is well known that the precision of results is roughly proportional to the focal length employed, and the difficulty of obtaining satisfactory plates somewhat in the same ratio. And what view should be taken of the practical merits of the scheme for determining proper motions by exposing a plate and putting it away for ten years, and exposing again? It has all the advantages of the similar plan for parallax determination, and more than the many difficulties of the latter—above all, the special difficulty that it can't be applied over intervals longer than a few years, and does not lend itself readily to combination with other measures of the past or the future.

These and many other topics of an equally delightful and difficult nature are discussed at the meetings of the Society of the Atlantic during the first week of its life, and one resolution is adopted without any dissentient—that the scheme which the member from Groningen first propounded at the St. Louis Congress is a grand scheme, and the Society will do its utmost to help him through with it. But the weather is getting hot, and we all feel with our President that "the intellectual activity is much diminished at sea," and there is a growing inclination to "study the higher mathematics," and we know that we shall have all the discussion over again when we get to the Cape, so for the present the formal meetings of the Society are suspended.

Today we have enjoyed a new sport—photographing astronomers without shadows. Perhaps it would be more correct to say that today *some* astronomers have no shadow to speak of. Of others the minimum shadow is really quite considerable, and not altogether attributable to the large hats which must be worn in the tropics. We have also remarked the peculiarly dead appearance of the sea when the Sun is right overhead. The complete absence of the wake of light is more noticeable than might have been expected.

The green flash has excited general interest, and some surprising observations have been made. A lady has seen a green flash extend almost to the zenith several minutes after the Sun went down, and it has been quite common to see the Sun reappear somewhat faintly, but distinctly green all over! It is, in fact, not quite easy to separate the real green flash from the effects of retinal fatigue, and the writer is not sure that he has ever succeeded. The last segment of the disappearing Sun certainly changes colour, but the result is not always a brilliant green. And two trained observers standing side by side do not get the same effects. A good deal can be done to guard against after-images and fatigue-effects in general by watching until the last moment the reflection of the Sun in a window instead of the Sun itself, or by getting a friend to give the signal for observation; but it is to be wished that some observer returning from an eclipse could set up his instrument and get the "green flash" spectrum.

There are many pretty things to be done with a good sea-horizon. For instance, the members from Groningen have been determining the size of the Earth by observing the difference in the time of sunset as seen from the main shrouds and from the lower deck. The method seems to bring out a result consistently too small, and it involves some rather nice points in refraction. But it led to the idea of a beautiful scheme for determining the figure of the Earth by measuring the zenith-distance of the sea-horizon in different azimuths from the Peak of Teneriffe. This is another project of the Senior Member from Groningen. He calculates that the difference between the dip of the south and of the east or west horizon is of the order of 20″. It is therefore within the bounds of possibility that observations of this kind might give a result of probable error comparable with the existing discordance between geodetic and pendulum determinations of the Earth's ellipticity, which differ by about one part in fifty. But there would be trouble again with the refraction, for the temperatures of the sea on a radius of 150 miles from Teneriffe are very likely to be persistently unsymmetrical.

Another amusement of the voyage has been to watch the shadow of the Earth rising in the east after sunset. On favourable occasions it appears as a segment of deep indigo-blue edged with the bright pink of the counterglow. One remembered that it had been photographed from the Peak by Mr. T. C. Porter some years ago, but it was a pleasant surprise to find the effect so conspicuous and beautiful. We have seen Mercury incredibly high above the western horizon, and the sport of finding Venus at midday has become so popular that it has spread to the firemen. In fact there has been a pleasant flavour of astronomy in our more sedate amusements; while the naturalists who promised much have had little to show but flying fish. There is one naturalist, however, who practises a charming method of collecting "plankton." He hangs a fine silk bag over a bath-tap and lets the sea-water run night and day. It is worth going down with him in the evening to see the bag full of phosphorescent beasts that shine when you stroke it like green lamps. "Oh, that's nothing," he remarks, "sometimes they're so bright that we pot them by their own light." It was that same naturalist who gave us the true theory of the migration of the lemmings. When they come down into the sea by myriads and are drowned, they are not following an ancestral fashion of migrating to Atlantis. They have come across country, eating it up as they go, and have got quite accustomed to swimming rivers. At last they come down to the sea. "Hullo!" they say, "here's one more river to cross!" and in they go.

The Society of the Atlantic has been having a few days' rest from its more serious labours and diversions, and has spent some delightful evenings gossiping. Some of the members have great stores of astronomical anecdotes, which defy recollection after a few days. But the member from Pulkowa has promised to write his reminiscences; and the promise is hereby recorded, lest he should forget.

How does it come about that the Fundamental Catalogue of the Astronomische Gesellschaft is not based upon the published Pulkowa Catalogue of 1865, but upon a preliminary draft of it? Who was it that missed a transit of Polaris through playing chess, and resolved never to touch the pieces again till the Moon's node had completed half a revolution? Who was the other man who was fond of showing his friends how he could set the meridian circle on Polaris in daylight without looking at the circles, till it was noticed that he always did it with a certain walking-stick in his hand? Identify the actors in the following story:—A and B resolved to publish an astronomical journal, whose leading principle should be enmity to C. A started off in the first number with an article on Comets. B followed in the second denying all his conclusions; and the leading principle was not strong enough to bring out a third number. Who was the diplomatic astronomer who set a telescope at random for the Minister of State, and gravely congratulated His Excellency on the discovery of a double star? This is a general knowledge paper that many of us would find it hard to answer till the promised reminiscences appear.

The Southern Cross has not appeared at its best, lying low on its side in the evening sky. It has even been spoken of disrespectfully, like the Equator, which proved to be cold. The fault does not lie with the Cross, but with the passenger by the fast mail steamer; that is the burden of a kindly reproof given by Admiral Wharton, whose death at the close of the expedition is our one sad memory of the whole. The early navigators, starting in spring, and creeping slowly down the coast, first sighted the Cross as it stood upright on the southern horizon, and their deeply religious souls were cheered night after night, as it stood little by little higher and higher, and seemed to lead them on towards the South. It is one of the finer experiences that we miss when we race South at the rate of six degrees a day. A. R. H.

The Meteors of Biela's Comet.

The probability of there being a brilliant display of these meteors during November 17–21 next renders the historical associations of the shower extremely interesting at the present time. My object in writing this note is not, however, to give a full review of past observations, but to point out that in recent years these Bielan meteors appear to have developed into a pretty strong annual shower.

The last abundant display from the region near the parent comet occurred in 1899, Nov. 24. At Vienna 90 meteors per hour were counted. Prof. Young, at Princeton, U.S.A., described the rate of apparition as 2 or 3 per minute. The shower was also witnessed at other American stations, at Munchen in Germany, and at Norwich in this country, where 52 Andromedids were recorded in $1^h 25^m$ (*Monthly Notices*, vol. lx. p. 374).

In 1902, Nov. 23–26, 23 Andromedids were seen at the University Observatory at Prag-Smichow, and Dr. Kavan contributed a paper on the subject to *Ast. Nach.* 3982. He gave the observed paths of 13 meteors of the shower seen close to their radiant-point. On projecting these on an 18-inch celestial globe I found the radiant diffused over several degrees with centre at 30° +39°. A few Andromedids were also seen at the same observatory in 1903, Nov. 25.

In 1903, Nov. 23 and 24, 25 Andromedids were recorded at the National Observatory at Athens by Prof. D. Eginitis. The details given in *Ast. Nach.* 4032 are as under:—

Date.	G.M.T.	Radiant-point.		Andromedids recorded.
		α	δ	
1903.	h m h m	°	°	
Nov. 23......	6 11 to 14 25	23	+43	14
24......	4 55 to 12 25	26 26	+46 +43	11

In 1904 the shower was well seen on Nov. 21 (though there was a full Moon shining in the sky) by the Rev. W. F. A. Ellison, of Enniscorthy, Ireland, and the radiant roughly estimated, from 40 or 50 tracks, was at 21° +50°. At Stockholm the display was similarly observed (*Astr. Nach.* 3997) on Nov. 21, the radiant-point of 28 meteors being found at 26° 2′ +43° 10′ (1900). The stream furnished some brilliant meteors and must have been very abundant to have attracted special notice in the presence of the full Moon.

Thus in each of the last three years the shower has been well observed, and, with the promise of a still more plentiful apparition, it should be thoroughly looked for in the third week of November next. The displacement in the node by the disturbing action of Jupiter will probably bring the shower back to Nov. 18, as pointed out some years ago by Dr. Schulhof, Prof. Abelmann, and others, but observations should be maintained between Nov. 17 and 23. The strength and particular day and hour of the coming display cannot be confidently indicated.

Bishopston, Bristol,
1905, Sept. 23.

W. F. Denning.

An Elementary Explanation of Recent Researches on Ancient Solar Eclipses *.

THE Moon's position in the sky is defined by its longitude and latitude, which are of course perpetually changing as the Moon moves. The latitude oscillates from $+5°$ to $-5°$, while the longitude continually increases at a nearly uniform rate, going once round (or 360°) in a month.

Hence approximately we have for the Moon

$$\begin{aligned} V \text{ (longitude)} &= nT, \\ U \text{ (latitude)} &= 18461'' \sin F, \end{aligned}$$

where T is the time, and where F is an angle that, like V, increases nearly uniformly with the time. You see from the formula that

$$U = +18461'' (= 5° \text{ nearly}) \text{ when } F = 90°$$

and

$$U = -18461'' (= -5° \text{ nearly}) \text{ when } F = 270°.$$

In my paper you will see a value for U given consisting of 14 terms. These are the more important "inequalities of latitude," as they are called. Their existence merely adds to the labour of calculating the Moon's place, but for the purposes of explanation we may suppose that the thirteen smallest terms do not exist.

Similarly, the Moon's longitude V may be considered as consisting of two parts:

$$L = nT + mT^2$$

and

$$V - L = 22 \text{ periodic terms},$$

all of which may be ignored for purposes of explanation, although of course account must be taken of them in the calculations.

You notice that I now write

$$L = nT + mT^2,$$

whereas at the beginning of the explanation I wrote

$$L = nT.$$

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* [In the supplementary number of the *Monthly Notices* of this year Mr. P. H. Cowell has published a paper deducing corrections to the secular variations of the Moon's mean motion and node from five ancient eclipses. These corrections had already been obtained from four out of the five eclipses when Mr. L. W. King, of the department of Egyptian and Assyrian Antiquities in the British Museum, applied to have a date assigned to an eclipse that was total at Babylon in the 11th century B.C. Now the present tables of the Moon do not make any eclipse total at Babylon within the required limits of time, but the corrections already obtained by Mr. Cowell do make the eclipse of −1062, July 31, total. In laying his conclusions before Mr. King, it was of course proper for Mr. Cowell to inform Mr. King that the totality of the eclipse in question does not follow from the tables generally accepted by astronomers, and, in a letter to Mr. King, Mr. Cowell has endeavoured to put his arguments into a form that can be followed by any one not especially acquainted with this branch of astronomy. When the letter was written, he offered the following extract from it to us, which we have gladly accepted.—EDS.]

It was Halley (in 1693) who first pointed out that the Moon's "mean motion" was not uniform—that is to say, that L cannot be represented by the value nT only, with sufficient accuracy. Down to about 1853 (that is to say for 160 years) it was supposed that the value of m, the coefficient of T^2, was considerably larger than it really is. Hansen, the author of the existing lunar tables, gave 12″ for its value. T denotes the time measured in centuries; therefore in -1062 T^2 is over 800, so that it makes a considerable difference whether its coefficient is 12″ or 7″; the difference is, in fact, 5″ × 800, or about one degree.

In 1853 Adams showed that theoretically the value of the "secular acceleration," as it is called, is a little over 6″. About 1878 Newcomb discussed the lunar eclipses mentioned by Ptolemy and deduced a value of 8″ for the secular acceleration. Nevill soon afterwards showed good reasons for the value of 7″. Nevill did not bring forward fresh material, but he treated Newcomb's material in a slightly different manner. In the same paper Newcomb said that he regarded the ancient solar eclipses as useless, and he has been repeating this opinion at intervals ever since, once so recently that I suppose he still holds it.

In the same way the Sun's longitude is calculated from a formula. The Sun's latitude is always zero, for latitude means "distance from the Sun's path," just as on the Earth it means "distance from the equator."

Now the places I have been writing about up till now are "geocentric" places, or the places as seen from the centre of the Earth. The place of the Moon as seen from Babylon is perceptibly different from its geocentric place, because the radius of the Earth is an appreciable fraction ($\frac{1}{60}$) of the Moon's distance. The difference between the geocentric and apparent place is called the "correction for parallax." Both the longitude and latitude require "correction for parallax." This adds to the length of the calculations without containing any essential point.

The Sun and the Moon, as you know, appear to be of nearly equal size. The Moon is just capable of covering the Sun, but there is very little to spare. Displace the Moon more than this very slight amount, and a portion of the Sun will be visible. Hence, if we are certain that an eclipse was total at a given place, we have a very exact observation of the Moon's position. Conversely, if the tables are altered so as to make two eclipses total, it is almost infinitely unlikely that the same alterations would fit on to three other eclipses. The only possible explanation is that the alterations of the table are correct, and that we happen to have heard of the eclipses in question because they were total.

I come to describe the nature of my alterations. As explained above, we have

$$L = nT + mT^2.$$

Similarly we have

$$F = qT + rT^2.$$

I have already alluded to the interest that has been taken for over two centuries in the value of *m*. The value of *r*, on the other hand, has never been questioned (except by Oppolzer and Ginzel, whom you will see mentioned in my paper; but although they both came very near to the truth in their ideas, yet they missed the mark entirely owing to, as is evident to Newcomb and me, a want of knowledge of the modern observations; but I will not go further with this digression).

It is evident that even a small alteration of *r* will make a great difference to F when T^2 is large, and a difference to F will affect the latitude of the Moon and will throw the eclipse-tracks northwards or southwards as the case may be.

Newcomb, *à propos* of Oppolzer and Ginzel's work, expressed his opinion that the value of L—F, or what we call the position of the node, must be considered as known.

My alterations are based on the hypothesis that both L and F must be treated as requiring independent corrections not linked together by the condition that the correction of L must be equal to the correction of F. This hypothesis works, it is consistent with five eclipses, and, moreover, the value of L deduced from solar eclipses is now nearly in accordance with Nevill's value from lunar eclipses, and with Adams's value from theory.

P. H. Cowell.

Special Evening Meeting of the Royal Astronomical Society.

As a sequel to the Meeting of the International Solar Research Union recorded in our last number, a Meeting was held at Burlington House on Wednesday, October 4, at 8.30 P.M., when Prof. George E. Hale, Director of the Solar Observatory of the Carnegie Institution of Washington, gave a lecture on "The Solar Observatory on Mount Wilson, California," at which there was, very naturally, a full attendance.

The lecturer, after remarking that he had not been at the Society's rooms since the last sun-spot maximum, opened his subject by describing the method of the spectroheliograph and showing results obtained with the instrument at the Yerkes Observatory. He then proceeded to describe the new Observatory at Mount Wilson.

This institution had its origin in a communication Dr. S. P. Langley made to the Carnegie Institution in 1902, recommending the establishment of an observatory at a very great altitude to study the solar radiation, which it was thought might be variable during a sun-spot cycle. A committee was appointed to make a report on the detailed requirements of a complete solar observatory and to choose a location. As the result of tests of atmospheric conditions at various sites in Southern California and Arizona, Prof. Hussey recommended Mount Wilson, near Pasadena and Los Angeles in South California, as the place, and here, thanks to the liberality of the Carnegie Trustees, the Observatory is being established.

As Prof. Hale remarked, in recent years telescopes have been growing, and spectroscopes have been growing, but as yet there has been no attempt made to bring together the best instrument of each class. It is hoped to do this at Mount Wilson, and the principal instrument for the purpose is the "Snow" telescope, so named after Miss Helen Snow, of Chicago, who gave $11,000 in memory of her father, which was sufficient to complete the telescope and instal it in a suitable house. This is a horizontal telescope, consisting of a 24-inch mirror of long focus fed by a coelostat, and is to be used in conjunction with a spectroheliograph, spectroscope, thermophile, or other apparatus for the study of sun-spots and solar phenomena. The observatory also possesses a reflecting telescope with 5-ft. mirror, mounted equatorially. A feature of the institution is the elaborate and well-equipped workshop under the charge of Mr. Ritchey, which is erected at the foot of the mountain at Pasadena, the difficulty of approach to the observatory making this arrangement advisable.

Prof. Hale showed photographs of the beautiful scenery around the observatory, and described the conditions of existence there. The place is called "the Monastery" because the staff live secluded on the mountain, leaving their wives and families at Pasadena or its neighbourhood, and visit their homes only at intervals.

The lecture was warmly applauded, the after speakers agreeing that the Society were fortunate in having this splendid institution and its work described to them by the man who had brought it into existence.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Biela and his Comet.

GENTLEMEN,—

The bodies suggested to our minds by the expression "November meteors" were principally, until a few years ago, the Leonids. But now that the most conspicuous part of that stream seems to have been withdrawn from our ken by planetary perturbations, the later stream called Andromedids (from its radiant) and Bielids (from the defunct comet with which it is connected) puts in for a higher place in our interest and attention. Of the comet itself, however, I purpose first to make a few remarks.

In the year 1772 Montaigne discovered at Limoges a comet on the 8th of March, which, according to Bessel's subsequent calculations, was moving in an elliptic orbit and had passed its perihelion on the 19th of February. Of Montaigne himself very little is known. He was born at Narbonne in 1716 and seems to have been a lawyer by profession. Carl spells his name Montagne, and whether he was connected in any way with the famous moralist of two centuries earlier (who was a native of Périgord) it is impossible to say. He afterwards discovered two other comets, in 1774 and in 1780, the latter (which was visible only for a few days) almost simultaneously with Olbers. The comet of 1772 was also observed at Paris; the last time on the 3rd of April.

Whether this comet was identical with that which afterwards acquired the name of Biela's is doubtful. But respecting that which was discovered by the great comet-discoverer Pons on the 10th of November, 1805 (almost exactly a century ago), and passed its perihelion on the 2nd of January, 1806, there is no doubt of its identity with the periodical comet which has not been seen (at any rate, as a comet) since 1852. This was pointed out both by Biela and Gambart, when they independently discovered it in 1826: Biela on the 27th of February and Gambart on the 9th of March. A priority of ten days therefore remains with Biela, and as this was the first appearance at which its periodicity was known, it is fitly called by his name. It passed its perihelion on that occasion on the 18th of March, and was followed until the end of April. The period was fixed at about six and a half years. Neither at this appearance nor at any subsequent one was it visible to the naked eye, as it had been in 1805.

Of Wilhelm von Biela himself not much can be said. He was born at Rossla, in the Harz, on the 19th of March, 1782, and served as captain in the Austrian army through the campaigns 1805 to 1809. After the peace he was appointed commandant of Rovigo, Venetia, and he died at Venice on the 18th of February, 1856. It was at Josephstadt, in Bohemia, that he discovered the comet.

Biela's comet reappeared in 1832, when it was first seen at Rome by Dumouchel on the 25th of August, and passed its perihelion on the 26th of November. Sir John Herschel observed it at Slough on the 24th of September, and remarked on the great advantages of an Italian sky that it should have been seen at Rome a month earlier. The last observation at that return was obtained at the Cape of Good Hope on the 3rd of January, 1833.

The comet was unfavourably placed at the return in the summer of 1839 and was not seen. It was at the next appearance, in 1845–6, that it acquired its great celebrity. Again it was first seen at Rome, by De Vico, on the 26th of November, 1845; but Dr. Galle (who, we rejoice to think, is still amongst us) perceived it two days later. The first person to notice its duplicity was Maury, at Washington, on the 13th of January, 1846; and two days later this extraordinary fact was perceived by Challis at Cambridge. But the investigations of Prof. Hubbard afterwards showed that the separation had probably really occurred in 1844.

In 1846 the comet passed its perihelion on the 11th of February and was last seen at Bonn on the 27th of April. The two divisions of the separated body had fluctuated in brightness, and great interest was of course felt at the next return in 1852. The comet was then again seen duplicated as before, the divisions being at a greater distance from each other than in 1846, and again fluctuating in relative brightness. Seen first by Secchi at Rome on the 25th of August, the perihelion passage took place on the 23rd of September, and the comet was last seen by Otto Struve (whose loss we recently had to lament) on the 28th of that month, but the fainter of the two nuclei was not discernible after the 25th, the increasing morning twilight leaving afterwards only the brighter one perceptible.

In 1859 the comet would have been very unfavourably placed for observation. It was not seen on that occasion, but was eagerly looked for at the subsequent return due in 1865-6. Neither then, however, nor since has it put in an appearance. But in 1872, when it would have been due again, lo! instead of a comet appeared on the 27th of November a splendid stream of meteors. Were these the remains of the disintegrated comet, which, after dividing into two, had further separated into a multitude of particles? Or might they be only a portion, and a larger one still deserve the name of Biela's comet? At the request of Klinkerfues, Pogson (then the Astronomer at Madras) looked in the direction opposite to that of the meteors for the retreating comet. He did notice one, but as its motion did not agree with that of Biela's, their identity is unlikely. However, less-marked showers of shooting-stars had previously been seen, also radiating from the constellation Andromeda. As the comet's motion was direct, so should that of the meteors be if there were really a connexion; this would bring their appearance earlier each year, instead of later like the Leonids, which, with their comet, move in a retrograde direction. And such appeared to be the case. Another very fine display took place on the 26th of November, 1885, when Biela's comet would have accomplished two revolutions since 1872. America was visited by a shower (smaller than that of 1885, but considerable) on the 23rd of November, 1892, which may, as Mr. Denning suggested, have been an associated branch of the main stream through which we passed in 1872 and 1885. The interesting question now is whether we are to be favoured with a fine display of Andromedids on that day or somewhat earlier next month.

Yours faithfully,

Blackheath, 1905, Oct. 4. W. T. Lynn.

OBSERVATORIES.

Paris.—M. Lœwy's Report for the year 1904, which we have not been able to notice before owing to the press of other matter, opens with sympathetic references to the losses that the Observatory has lately sustained. The deaths of MM. Callandreau and Paul Henry have already been mentioned in these pages, and to these there must be added that of M. Barré, one of the oldest assistant-astronomers, who died suddenly from congestion of the brain.

With the Grand Instrument méridien, 7553 observations were made during the year. With the Gambey meridian instruments 3331. Transits are now registered chronographically at Paris, the registration of observations with the Gambey instruments being made by means of a printing chronograph designed by M. l'Abbé Verschaffel.

The Cercle méridien du jardin has been the subject of some investigation. This instrument has now two reading-circles, and it was found that the measures of the same angle made by the two circles differed by as much as 0″·45. It was thought at first that this was caused by flexure of the circles due to dissymmetry of the spokes, but investigation showed that the cause of the discordance was optical. The same division of the circle did not look the same when seen under each of the six microscopes, owing to a fault in the lighting. This has been remedied: the object-glasses of the microscopes on the west pier have been replaced by larger ones of a greater focal length, and some of the worn divisions have been repaired. These alterations appear to have got rid of the discordance above mentioned. It has been decided to replace the divided silver circles by others of an alloy of platinum and iridium. This instrument has undergone many improvements and has been supplied with many additions since its inception. When this last improvement has been made it is thought that it will be fit for researches of great precision.

A photographic meridian telescope, after the design of M. Lippmann, has been installed, but observations with this are as yet not mentioned.

M. Bigourdan's observations of nebulæ with the equatorial in the western tower have been temporarily stopped, so as to make way for a research on the constant of aberration by a new method due to M. Lœwy. The method consists in making the images of two pairs of stars appear in the field of view at the same time. This is done by the use of a double mirror cut in a single block of glass. The stars of each pair are separated respectively by 90° in space, and the two pairs are so chosen that aberration increases the distance between the stars of one pair and diminishes the distance between the stars of the other. The measured distances are compared with similar measures made at a time of year when the phenomenon is reversed. A new opening in the dome has of course had to be made for this.

A paragraph in the section relating to the reduction of the Astrographic Catalogue plates comes rather as a surprise. It has been decided that the plates shall all be remeasured in the reversed position to obtain greater precision, *i. e.* with the plate turned 180° from its first position. This plan has been adopted almost from the beginning at Greenwich, apparently with advantage, and also at Oxford. It will probably delay the publication of the Paris Catalogue, of which one volume is already issued, although the remeasurement is proceeding apace.

Astronomical physics is to be added to the Observatory scheme of work, but the spectroscope which is to be used on the Great Equatorial Coudé was not finished at the date of the report. Various laboratory researches in preparation had been made during the year.

M. Hamy, *astronome-adjoint*, has been promoted to the rank of *astronome-titulaire* in place of M. Callandreau.

NOTES.

Comet Notes.—Dr. F. E. Ross has computed a new orbit of Comet 1844 II. (Mauvais). As the comet was under observation for eight months and had a well-defined stellar nucleus whose position could be accurately determined, and as errors had been found in the planetary perturbations computed by Plantamour, a revision of the elements was considered desirable, using Newcomb's 'Tables of the Sun,' and determining proper motions for the comparison stars.

Adopted orbit.

T	1844 Oct. 17·3801 ±0·0002 Berlin M.T.	
π	242° 54′ 2″ ±3″	1845·0.
☊	31 39 3 ±2	
i	131 24 3 ±1	
$\log q$	9·932163 ±0·000001	
e	0·999598 ±0·000008	
Period	98000 years ±3000.	

In August 1845 the Comet passed within 2 units of Jupiter. Dr. Ross proposes to investigate how far the period was affected.

A. C. D. C.

Minor Planet Notes.—The following planets have received permanent numbers:—

Planet.	Number.	Name.	Planet.	Number.	Name.
PS	554	Peraga.	QH	562	
PT	555		QK	563	
PW	556		QM	564	
PY	557		QN	565	
QB	558		QO	566	
QD	559		QP	567	
QF	560		QS	568	
QG	561		QT	569	Misa.

The following identities have been established :—

PU ..	489 Comacina (probably).	QJ ..	480 (probably).
PV ..	149 Medusa (missing since 1875).	566 ..	JF (discovered 1899).
PX ..	517.	QR ..	216 Kleopatra.
QC ..	427.	QU ..	406.
QE ..	311 Claudia.	QV ..	263 Dresda.
		QY ..	167 Urda.

The following do not receive permanent numbers, not having been sufficiently observed :—PQ, PR, PZ, QA, QL, QQ, QW.

A. C. D. C.

Obituary.—With deep regret we have to announce the death of the Rev. S. J. Johnson, a frequent and valued contributor to this magazine.

Samuel Jenkins Johnson was the only son of the late Rev. S. Johnson, sometime Vicar of Atherton, Lancashire. He was born on March 14, 1845, and after leaving school entered at St. John's College, Oxford, and took his degree in 1866. He was ordained at Manchester in 1868, and after serving in two curacies in Lancashire, was presented to Upton Helions, Devon, which he held from 1870–79. For two years he occupied the Rectory of Abbenhall, near Gloucester, and in 1881 went to Melplash, Bridport, Dorset, which vicarage he held until his death. The deceased gentleman was elected a Fellow of the Royal Astronomical Society in 1872 March, and published his first paper to the Society in the Supplementary Number of the 'Monthly Notices' for the same year, entitled "On Future Solar Eclipses," which gave a list of eclipses in the 20th, 21st, and 22nd centuries. This was a subject which he made peculiarly his own, and his book 'Eclipses Past and Future,' first published in 1874, and republished with additions in 1895, is famous. He devoted himself also to calculating the dates of occurrence of unusual phenomena, such as those he gave in an article published in the Report of the Liverpool Astronomical Society shortly before his death, where he noted the occultation of Mercury by Venus in 1737, which is a unique example of the telescopic observation of an occultation of one planet by another, and an occultation of Mercury by the Moon, which happened on 1838 April 25. In the same place he announced that he had just seen Mercury with the naked eye for the hundred and fiftieth time, all of which well illustrate the kind of astronomy that the Rev. S. J. Johnson followed. In 1882 he went to Marseilles to observe the Transit of Venus, where the sky was partially cloudy. In 1900 he had an excellent view of the solar eclipse from a place south of Madrid, and he went to Burgos in August last with not such a successful result. Since his return from Spain he has not been well, and was ordered complete rest.

He, however, attended a Church Congress at Weymouth a few days before his death, but was too ill to take service on Sunday, October 8, and passed away on Monday morning from heart-failure. The deceased leaves a son and daughter, his wife having died about twelve months earlier.

WALTER F. WISLICENUS. There is also occasion to record the death of this astronomer, a Professor in the University of Strasburg, which took place on October 3.

Walter Wislicenus was born on 1859 November 5, was educated at Dresden and at the Universities of Leipzig and Strasburg. In 1882 he went with one of the German expeditions to observe the Transit of Venus, and in 1883 joined the staff of the Strasburg University Observatory. In 1884 he was made an Assistant, and was entrusted with the work of observing with the new Repsold meridian-circle, which he did for some years. Dr. Wislicenus has contributed much to the literature of astronomy. A paper by him "On the Rotation-Period of the Planet Mars" appeared in 1886, followed shortly by another on "Personal Equation in Transit Observations," but later on he devoted himself to compiling books on some branch of the science; a series on Astrophotometry, Astrospectroscopy, and Chronology are by him, and he wrote a good deal on geography and elementary astronomy; but specially he will be remembered for his useful 'Astronomische Jahresbericht,' which formed an index—and a very complete one—to all astronomical literature which appeared during each year. This was begun in 1900, so that five volumes have already appeared. It is to be hoped that this will not stop with his death.

DR. RALPH COPELAND, Astronomer Royal for Scotland, died on Friday, October 27, at the age of 68. A notice of the life and work of this gentleman will be given in our next number.

SUN-SPOTS IN OCTOBER. — Though this is said to be a "maximum" year, the Sun has not been especially prolific in large spots as to their number, but the year is remarkable for the number of spots of extraordinarily large size. For some days in October there were two spots on the disc, both visible to the naked eye. The first, which covered a total area of 2400 millionths of the visible hemisphere and was about 16° of solar longitude in length, came round the limb on October 14, and crossed the central meridian on October 20. The heliocentric position of this spot is given by longitude 160°, latitude +13°. The second spot, which was not so large and in about the same latitude and longitude 60°, came into view on Saturday, October 21, and crossed the central meridian on October 28. These two bring the number of spots seen during the year with the naked eye to six. There have been no disturbances of the magnets at Greenwich of any magnitude.

A Cold Dry October.—Until the last week in the month it seemed quite likely that we should have to report the greatest number of days of frost, the lowest mean temperature, and the smallest rainfall, in the 65 years of Greenwich records. But as so often happens, and as happened notably in 1888 and 1892, the last week brought a pronounced change, and October 1905 only takes rank among the half-dozen or so coldest, driest, and frostiest Octobers, somewhat approximating in this respect to October 1887. It may be noted that cold Octobers have been more frequent in the latter half of the period, there having been no case between 1850 and 1880 of a mean temperature 3° below the 50 years' average (the 65 years' average is practically the same), whereas including 1880 and 1905 there have been *nine* such Octobers since that time, to compare with only two, 1842 and 1850, in the first part of the period.

In the light of Dr. Van Rijckevorsel's investigations on the subject of secondary maxima and minima during the year, it will be interesting to see how far other records confirm the apparent secondary maximum about Oct. 27.

Since the three driest Octobers, 1861, 1879, and 1897, were not cold, and since some of the very cold Octobers were distinctly wet, that of 1880 being not only the wettest October but the wettest month ever recorded here, we may say that the October just concluded has been decidedly abnormal. W. W. B.

Reports of the Solar Eclipse.—The Joint Eclipse Committee of the Royal Society and Royal Astronomical Society held a Meeting at Burlington House in the rooms of the former on October 19, to receive the reports of the observers of the Total Solar Eclipse of August 30 last. The Astronomer Royal gave details of the expedition, under his leadership, to Sfax in Tunis, and showed a series of photographs of the inner corona and prominences taken by himself with the Thompson 9-inch coronagraph and by Mr. Davidson with the lens of the Astrographic telescope of 13 inches aperture. Owing to the cloudiness of the sky during the time of the eclipse, the whole length of the streamers was not seen, in fact they were not traced on the photographs to a distance of more than two solar diameters from the limb. The Astronomer Royal pointed out an elliptical shaped formation in the corona which gave the idea of its being the result of an explosion, and also said that it seemed that the dark rifts in the corona appeared to be actually dark matter and not merely interstices between bright rays.

Mr. Dyson, who was with the Astronomer Royal at Sfax, showed a spectrum photograph. The slit of the spectroscope was adjusted so as to be tangential to the Sun's limb at the point of second contact, and the plate therefore showed a spectrum of the corona and of the large prominence which was a little to the north of the Sun's east point. The spectrum of the first showed two lines, one at λ 5117, the other at λ 5536, not hitherto seen in the coronal spectrum.

Mr. Evershed described the details of his equipment and location at some length, but the sky at his station in the neighbourhood of Burgos was completely overcast at the time of totality and he has no results to show.

Prof. Callendar showed diagrams and gave explanations of the apparatus he had installed at Oropesa, on the east coast of Spain, for measuring the solar radiation and the air temperature during the eclipse, but here the Sun became covered with cloud soon after first contact. The cloud remained during totality, but cleared before the eclipse was over. The recording thermometer showed that the temperature fell about 3½° C. between the time of first contact and the middle of the eclipse. Mr. Fowler, who was with Prof. Callendar, observed the prominences round the limb of the Sun before the eclipse.

Mr. Newall had been stationed at Guelma, in Algiers. The place had been chosen because the meteorological records promised a fine sky. It happened that between one and two o'clock, the time of the eclipse, on each day that the observers were there it was either overcast or stormy, except on the day of the eclipse, when the sky was all that could be wished. Mr. Newall traced the streamers by unaided eye to some distance from the limb—the streamer near the south pole to between five and six solar diameters. The corona was described as being profuse all round the Sun, and was therefore of the type associated with the maximum of sun-spots. There were no signs of polar plumes or equatorial streamers. Mr. Newall supported the Astronomer Royal's opinion that the dark markings were actual absorbing matter and urged that it was almost impossible to suppose them to be "rifts" in a three-dimensional corona. This speaker ended by giving an explanation of the polariscopic results obtained at Guelma.

Prof. Turner described the personnel and station of the observing party at Assuan, and showed the photographs of the corona taken in polarized light by reflection, the reflecting mirror being placed in each of two planes at right angles.

Time did not allow of any discussion. The Meeting was brought to a close by some words of congratulation from Mr. Maw, the President of the Royal Astronomical Society, who occupied a seat on the platform, and from Sir William Huggins.

THE FIGURE OF THE SUN.—The result of a research made by Dr. C. L. Poor, and published in the *Astrophysical Journal* for

September last, would be remarkable and a valuable addition to knowledge if it were well founded, for it is claimed that the investigation shows that the ratio between the polar and equatorial radii of the Sun is variable, and that the period of this variability is the same as the sun-spot period, but it is feared that the data are insufficient to warrant such a bold conclusion. Dr. Poor took certain Rutherfurd photographs of the Sun—four taken in 1870, eight in 1871, and ten in 1872—and found the quantity Polar—Equatorial diameter for each, the measures naturally being corrected for refraction and otherwise treated with discrimination. It results that the value of this difference for the three years respectively is $+0''\cdot50$, $-0''\cdot32$, $+0''\cdot22$, and it is argued that this shows a real progressive change. But this seeems hardly warranted, because—as, indeed, Dr. Poor points out—the different plates in each year give consistent results (there are, however, *some* discordances), although the results differ; so that it appears that there was little change in the Sun's form between August and October 1870, but that there was a change between this date and 1871 April 21 of more than $1''$ in the quantity P—E, which then remained constant for four months, but changed *per saltum* again by about the above-named quantity back to its original value; and as it happened that 1870–71 was a sun-spot maximum, it will be seen that if these three values of P—E are plotted, the curve is not unlike the sun-spot curve at this time; but it seems to us that this gives an altogether undue importance to the measures of the solar photographs in the summer of 1871, the results of which might be abnormal from some other cause, though this may not yet be known.

Other data are used in the investigation. There are five photographs of the Sun taken at Northfield (three in 1893, two in 1894), which are treated in the same way. The resulting Polar—Equatorial diameter found from the two plates taken on July 10 and July 17, 1894, differ by $1''$, but their mean is taken to prove that this difference of diameter changed by $0''\cdot67$ between 1893 September and 1894 July, and this change, though depending on such slender evidence, boldly plotted beside the sun-spot curve, with which it agrees in direction, gives to the casual glance somewhat convincing proof of the hypothesis. But we fear it will not stand cross-examination.

A NOVA IN AQUILA.—A telegram from Kiel on September 1 announced the discovery of Nova Aquilæ No. 2. It appears that this object was discovered on August 31 by Mrs. Fleming when examining a spectrum photograph in one of the Draper Memorial Series taken on August 18. The star was then of magnitude 6·5, and its spectrum showed the hydrogen lines $H\delta$, $H\gamma$, and $H\beta$ bright and broad, and also faint traces of the bright bands at λ 4472 and λ 4646. On August 21 its magnitude was 7·5, on August 26 it was 10·0, and it was observed by Prof. Hartwig on September 5 still of this magnitude. The Nova was not shown on a plate taken at Arequipa with the Bruce telescope on which sixteenth-magnitude stars are shown. The place of the Nova is given by R.A. 18h 57m 5s, Dec. −4° 35′, from which it will be seen that the object is in the Milky Way, not far from λ Aquilæ. Nova Aquilæ No. 1 was discovered by Mrs. Fleming in 1900 July.

Among other distinguished visitors at Greenwich lately we have been glad to see Prof. W. J. Hussey, late of the Lick Observatory, who has been appointed Director of the Ann-Arbor Observatory, Michigan (Detroit Observatory). Also Prof. W. W. Campbell, Director, and Dr. and Mrs. Perrine, of the Lick Observatory. Prof. Eichelberger and others who attended the Conference at Oxford in September have also visited Greenwich.

The Janssen Medal of the French Astronomical Society has been awarded to Prof. Lowell, of the Massachusetts Institute of Technology, for his work on Mars. It may be added that this "work" does not include the photographing of the canals of which an illustration was given in the August number of this magazine, for the award was made previously to the taking of these photographs.

Mr. W. T. Lynn's little books, 'Astronomy for the Young' and 'The Penny Chronology,' are too well known to need further commendation. We have received copies of new editions of both of these. The last entry in the last-named relates to the peace-treaty between Russia and Japan, so it is thoroughly up-to-date. Mr. Lynn's publishers are now Messrs. Bagster & Sons, Paternoster Row.

The telescopes and instruments of the late Dr. Isaac Roberts were sold by auction by Mr. J. C. Stevens on October 17. The equatorial with the 20-inch reflecting telescope and the 7-inch Cooke refractor are now in the possession of Mr. James Bower, an amateur astronomer of Norwich.

There will be a convenient occultation of Aldebaran on the evening of Monday, November 13, disappearance at 6h 35m G.M.T.

The tenth satellite of Saturn has been named Themis, a name already appropriated to a small planet. The seventh satellite of Jupiter has been photographed with the 30-inch reflector at Greenwich.

The next Meeting of the Royal Astronomical Society will be on Friday, November 10; of the British Astronomical Association on Wednesday, November 29. The Annual Council Dinner of the Association will be held after the Meeting, and all Members who observed the total solar eclipse are invited to attend.

FROM AN OXFORD NOTE-BOOK.

P. & O. S S. 'Egypt,'
1905, Sept. 15.

"THE Eclipse has occurred," as Reuter once telegraphed. The climate of Egypt did not betray us: we had a splendid day, we have developed our plates and found sufficient to be thankful for, and we are all on the way homewards. I hope all those who were working so earnestly side by side a few weeks ago and are now dispersed in different directions are having as satisfactory a voyage as we—perfect weather in a large comfortable boat. The company is, perhaps, a little small—it is not the best time of year for company; but then we can say, with the man who had lost a scolding wife, "It's perhaps a little dull, but it's very peaceful."

THE last days at Aswan were indeed calculated to make us appreciate peacefulness, for they were busy and anxious. Mr. Reynolds's big 120-foot telescope arrived four days before the eclipse, and when it arrived it was found that the Sun at eclipse time was just too low to bring into the instrument as it stood. The coelostat mirror was so mounted that it could not be completely rotated on its axis. Mr. Reynolds had, in fact, considered it dangerous to turn a large 28-inch mirror face downwards with only edge-suspension, and the stops arranged to guard against this did their work a little too well. Various remedies were discussed. One was to chip a piece out of the cast-iron backbone of the coelostat; and this was actually done. The man who sat for a morning under a nearly vertical Sun chipping away cast-iron is proud to reckon the transaction among the notable achievements of his life. But it was not enough; more was needed, and the further question arose whether this should be exacted from the coelostat itself or from one of the piers. When bathing our weary limbs in the Nile after sunset we were interested to see that the maker of the telescope and the builder of the pier were discussing from which to take toll under somewhat novel conditions: they were seated on the soft bed of the river, their heads alone above the water, and were advancing in turn the claims of their respective children with great earnestness. When all other bathers had left the river, the two heads were still there and still talking, and they made a striking picture in the growing darkness. It is pleasant to be able to record that the discussion ended harmoniously with a decision to alter *both* pier and telescope.

THE operation on the backbone of the coelostat was not the only surgical operation performed at Aswan. On attempting to focus the four telescopes of the "cross-eyed lady" it was found that the tube of one of them had by some mistake been cut too long. The diagnosis was not made hastily nor acted upon without a consultation, but it was finally decided to amputate a portion of the tube. The operation was performed with great skill by Dr. Hussey in his open-air theatre on the banks of the Nile, and in the presence of an admiring crowd of students and fellow-surgeons. Optical tests applied at night proved that the myopia had been completely cured.

On the day before the eclipse a large steamer appeared, bringing Mr. Mitchell-Innes, who represented the Egyptian Government and accorded a very cordial and pleasant official welcome to the expeditions. Nevertheless, the advent of his steamer caused a little alarm to those making magnetic observations near the river, though their anxieties proved ultimately to be needless, for the traces on the registers were not disturbed. But another effect of the presence of these steamers is worthy of attention: their wash was found sensibly to shake the banks, and the Sun's image in Mr. Reynolds's big telescope quivered alarmingly at their approach. On this being made known, the authorities very kindly stopped the steamers during totality, so that no actual inconvenience was suffered. But the whole question of the erection of eclipse instruments near large masses of water requires consideration and experiment. Some time ago Mr. Dyson told me that at high tide he noticed considerable tremor in instruments erected near the sea-coast; at low tide it disappeared, and fortunately the eclipse took place at low tide, so that he was not inconvenienced. But this would not always happen, and, unless precautions are taken, some day valuable observations may be spoiled by the beat of the waves or tremor of the banks of a great river. For obvious reasons, eclipse instruments are specially likely to be set up near seas or rivers, unless there is good reason to the contrary.

The natives were much interested in our operations, and regarded the most trivial part of our equipment with reverence. Once we were bringing ashore a heavy wooden wedge not bearing any obvious mark of astronomical purpose, for we could scarcely expect natives to observe that it was cut to the latitude-angle. There was quite a brisk competition for the handling of it when the boat touched the landing-place, and I may add that no question of payment was involved. When the actual eclipse came they were impressed, and the women gave the joy-cry, but they were clearly not in the least taken by surprise.

Probably most of us know the famous solar eclipse in Rider Haggard's 'King Solomon's Mines,' where it was totally dark, so dark that people could only blindly grope their way, for about two hours. In later editions the passage has been modified, but in the first edition it was most amusing reading. The portent was, of course, used with great effect to frighten the natives. In reading it many years ago for the first time, I remembered a very similar instance in another book of African travel, a boy's book called 'Cast up by the Sea,' by Sir Samuel Baker. It may be worth while to compare the two chapters side by side (see below), though of course the second was not necessarily suggested by the first. Indeed, since coming to Egypt I have come across what may be the origin of both—a passage in the 'Travels of James Bruce of Kinnaird, Esq., F.R.S." The travels were in Abyssinia about 1770, and Mr. Bruce discovered the source of the Blue Nile. When he came back and wrote his book it was parodied by the famous 'Adventures of Baron Munchausen'; and yet most of it at any rate was good solid fact, of which confimation is still coming to hand. But we are concerned with his mention of an eclipse. It was a lunar eclipse this time, and a real one, that of Friday, April 11, 1772. He used it as a threat, and this is his account of what happened:—

> I had rectified my watch by observation, and I knew I could not be far wrong, having seen in the ephemerides the hour the eclipse was to begin. . . . . I went to the door and saw it was begun. There was to be a total eclipse of the Moon. I did not tell them at first till the Moon having arisen the shade appeared some way advanced upon the disk. "Now look at that," said I, "in some time after this the Moon shall be so totally swallowed up in darkness, that a small light only shall be seen in the edges," &c., &c.

Now the curious thing is that, as Mr. Bruce's Editor in 1805 remarks, the Moon *rose totally eclipsed* at Teawa. The Editor assumes that Mr. Bruce's memory, in writing of these events some years afterwards, betrayed him as to the details, and we may accept this view. We then have to contemplate the fact that a man who was keenly interested in being accurate, a Fellow of that Royal Society which more than a century earlier had been founded for the express purpose of sifting accurate information from the rubbish which had accumulated—a man, moreover, who had consulted ephemerides and knew that they could be used to check his assertions—such a man could deliberately make this erroneous record about an eclipse which he had actually seen upon a stirring occasion. Is it to be wondered at that the accounts of ancient eclipses, when there were no such incentives to accuracy, are worth so little?

It would seem to be the part of a prudent man to evade describing an eclipse unless he can fasten the responsibility of the description on others. Hence I shall quote Prof. West, of Beyrout, as saying that the longest streamer, about 1½ diameters, seemed to him to be associated with the big sun-spot that had just come into view; and various other people for the opinion that there was no wind to speak of, no fall of temperature, and no sight of Mercury. For myself I was, as a matter of fact, watching and regulating the clock which was driving two coelostats. The experience was a new one, but it suggested forcibly that it is worth including definitely among the things to be done. The clock-rate did not change much, but it did change, and I am sure that I was able to secure better clock-driving than if I had not been there watching the image and adjusting the regulator accordingly. The duty has an incidental advantage which will doubtless commend it to others as to me—it keeps one occupied through those rather trying minutes just preceding totality. After totality we were able to see the corona for ten or twenty seconds, and to trace the Moon's limb on the corona for a full minute; but none of us could see corona before totality, the last bit of the Sun seemed so dazzling.

We were invited to a very pleasant dinner in the evening under the presidency of Mr. Mitchell-Innes. The tables were put out on the croquet-lawn and gaily decorated. All the expeditions were welcomed in most courteous terms, and we were all glad to have an opportunity of gratefully acknowledging the invaluable help received from Captain Lyons and the Survey Department. And so ended most happily a day which can never be anything but arduous.

But not, unfortunately, for all of us. One member of the party had a most unwelcome toil yet to come. We had all decided to defer developing plates for at least one night, for we had all been at work since an early hour in the morning, and were weary. But we had reckoned without a certain gentleman who had arrived at the last moment, stuck up a small telescope on a post without clockwork, and taken some photographs. They were not bad photographs under the circumstances, but they were scarcely so important as he thought them. For him they seemed to take precedence of everything else, and he was not satisfied until a poor tired astronomer, who had been busy with really serious matters all day, had helped him to develop the last of his plates about 2 A.M. It was so gross a case of selfishness that it ought to be mentioned as a public warning. Those people who only wish to amuse themselves at an eclipse ought really to be more considerate towards those who have important work to do.

Next night we were able to photograph the New Moon just as it was setting. It would, perhaps, have been difficult to utilize the observation so as to obtain the place of the Moon; but on the following night there was no difficulty at all. Good photographs were obtained, showing the earth-shine. It seems clear that the Moon could be photographed regularly all round the lunation in Egypt, with the exception of two or three days at most, and that the monthly inequalities in the motion could be determined in this way with great accuracy.

An interesting diagram might have been made of the influx of visitors to the hotel. We started as a party of ten, and remained stationary at that figure for many days; then came the helpers, who doubled our numbers, and on August 29 we went up with a bound to over 70. The crowd dispersed much as it came, and in a few days the original ten, or a near equivalent, were left, but the curve did not continue to repeat in the inverse order, for the ten soon dwindled to three or four. We were ourselves among the deserters; the last to "leave the ship" was Dr. Doubinski, who went on placidly working in his tomb almost as long after the eclipse as before it, and with equally good reason.

We deserted Aswan for Cairo in order to see our plates developed in the magnificent dark-rooms of the Survey Department and in the able hands of Mr. Kearney. We lived luxuriously in a dahabiah while this was being done, and learnt a little more of life in Egypt under its normal aspect. There were many delightful things to be done there. We were invited to dine with a club formed for the express purpose of "talking shop," and are still wondering whether we acquitted ourselves satisfactorily. There were the pyramids to see, and we learnt of the fascinating scheme for taking pendulum observations on the top of the Great Pyramid and again within it, for determining the mass of the Earth, with latitude observations north and south of it as a possible supplement and confirmation. And there was the Helwan Observatory to visit, where Mr. Reynolds was heroically struggling to get his 30-inch reflector erected in the limited time he could stay in Egypt.

But all these good things came to an end. Our plates were developed and copied without mishap, and though we had a serious disappointment in those of the long 120-foot telescope, which were badly marked by the wood-work of the plate-holders, we had much else to be thankful for. We found ourselves in due course at Port Said on this good ship 'Egypt,' in spite of a little accident in the Suez Canal the week before, which resulted in the waterway being embarrassed by a sunken ship full of dynamite. We have not yet heard the subsequent history of that ship, which could not fail to be interesting, seeing that dynamite becomes more dangerous with long immersion. But we are hoping to hear this and other news to-morrow when we reach Marseilles.

Will it hurt anyone's feelings if I venture to describe Egypt as in some ways a ridiculous country? It is not that it is built in one dimension instead of two; that it wears the aspect of a barren desert (in the summer at least) and is really the most fertile country in the world; that its mud is cut up into territories little bigger than a developing-tray; but it has such ridiculous animals. There are sheep and goats who deliberately pretend to graze where there is not a blade of grass in sight—not a green thing except the tops of the palm-trees which they cannot reach. And then the water-buffalo! and above all the camel!! The ass has been a byword for stupidity since the days of Balaam, but in Egypt his intelligence would be of a high order by comparison. He does not, like the water-buffalo, linger on the railway-line until the engine knocks out of him the little sense that isn't there; and you have only to compare an ass with a camel in appearance, as you may when you see them ploughing together, to realize at once that the former is immensely superior in intellect. And when we come to behaviour, the donkey does not, as the camel does, complain to the whole world while he is being loaded and then walk off with his burden as meekly as possible. Something, of course, depends on the point of view; the camel no doubt regards himself as correct in appearance and behaviour, and laughs at human beings. But I cannot but think that Swift would have had a harder task if he had tried to make a Houynhm with a camel instead of a horse.

Nevertheless, Egypt is also a most fascinating country. We saw it almost at its hottest and driest, when the eye meets no restful green; as we are leaving it the river is rising, and some of the country which we saw as a desert is now a vast lake. In a few months brown will be replaced by green, and the whole land will wear a totally new aspect, and nothing would please us better than to have a valid excuse for seeing it in the green tree as we have seen it in the dry.

Appendix.

It may be of interest to add here the actual extracts above mentioned which deal with eclipses.

From 'King Solomon's Mines,' by H. Rider Haggard (Cassell & Co., 1885):—

> I glanced up at the Sun, and to my intense joy saw that we had made no mistake. On the edge of its brilliant surface was a faint rim of shadow. (p. 185.)

* * * *

> Slowly the dark rim crept on over the blazing surface, and as it did so I heard a deep gasp of fear rise from the multitude around.

* * * *

Meanwhile the dark ring crept on. Strange and unholy shadows encroached upon the sunlight, an ominous quiet filled the place, the birds chirped out frightened notes, and then were still; only the cocks began to crow.

On, yet on, crept the ring of darkness; it was now more than half over the reddening orb. The air grew thick and dusky. On, yet on, till we could scarcely see the fierce faces of the group before us.

* * * *

"Now, chiefs," I said, "we have given you the sign. If ye are satisfied, let us fly swiftly to the place ye spoke of. The charm cannot now be stopped. It will work for an hour. Let us take advantage of the darkness."

* * * *

Before we reached the gate of the kraal the Sun went out altogether.

Holding each other by the hand we stumbled on through the darkness.

CHAPTER XII.

Before the Battle.

Luckily for us, Infadoos and the chiefs knew all the pathways of the great town perfectly; so that notwithstanding the intense gloom we made fair progress.

For an hour or more we journeyed on, till at length the eclipse began to pass, and that edge of the Sun which had disappeared the first became again visible. In another five minutes there was sufficient light to see our whereabouts, &c.

From 'Cast up by the Sea,' by Sir Samuel Baker, M.A., F.R.S., F.R.G.S; 1874 (pp. 323–5):—

Ned himself began to think he had made some error in the calculation of the hour, as the shadow of the lance almost reached the line, when a sudden gloom, as though a passing cloud, caused the shadow to disappear. Ned sprang from the ground, and raising his extended arms towards the heavens, he invoked the protection of the Almighty in a loud clear voice that produced a startling effect upon the King.

The gloom increased, and, shortly, a black shadow veiled a portion of the Sun, and a dark sphere crept slowly but surely over its disc. It was like the dim twilight of evening gradually increasing towards darkness, and the round black object could now be seen creeping across the Sun until merely a thin crescent of its light remained.

With a loud cry the King and all his people threw themselves upon the ground, and hid their faces in their hands; Ned and Tim alone remained standing amidst the crowd of prostrate savages.

"Cry out, Tim, in your loudest voice," said Ned, "This is the work of the Great Spirit that the white man serves! this is the hand of his God."

Tim repeated these words with great energy in the native tongue: the whole crowd groaned with terror.

"Cry out again, Tim," said Ned, "Rise up, and the Sun will regain its light."

At these words the King arose trembling, together with some of his people, but the greater part still remained prostrate upon the earth. Ned then approached the King and directed his attention to the Sun, which slowly resumed its brilliant proportions as the eclipse passed away.

Turning to the chief executioner, the King gave some directions, and, before Ned could interfere, the sorcerers who had advised his death were seized and strangled.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1905 November 10.

Mr. W. H. Maw, *President*, in the Chair.

Secretaries: E. T. Whittaker, M.A., and
T. Lewis.

The Minutes of the previous Meeting were read and confirmed.

Mr. Lewis. One hundred and eighty-two presents have been received since the last Meeting. Among those calling for special mention are the books:—Miss Clerke's 'System of the Stars,' 2nd edition, presented by the author; Galileo, 'Opere,' Edizio Nazionale, vol. 16, presented by the Italian Government; 'New Reduction of Groombridge's Circumpolar Catalogue,' by Messrs. Dyson and Thackeray, presented by the Royal Observatory, Greenwich; G. W. Hill's 'Collected Mathematical Works,' presented by the author; 'Drawings of Mars, 1903,' presented by Prof. Percival Lowell; A. Marcuse, 'Handbuch der geographischen Ortsbestimmung,' presented by the author; Translation of G. V. Schiaparelli's 'Astronomy in the Old Testament,' presented by the Clarendon Press, Oxford.

The Society has also received continuations of the series of the Astrographic Chart enlargements from the Royal Observatory, Greenwich, and from the Paris Observatory; photographs of the Seventh Satellite of Jupiter taken at the Lick Observatory, presented by Prof. Perrine; photographs of instruments and of the eclipse of August 30, presented by Rev. R. Killip; portrait (photograph) of Prof. Lœwy, presented by Prof. Turner; photograph of the Lacaille Memorial at the Cape, presented by Mr. Hinks; and a small ring dial, presented by Mr. Ellis.

A vote of thanks was accorded to the donors of the presents, and the President called upon the Astronomer Royal to open the

Meeting by showing the photographs of Jupiter's satellites taken at Greenwich.

The Astronomer Royal. In bringing forward these photographs of the sixth and seventh satellites of Jupiter I wish first to express my sense of the credit which is due to Mr. Melotte for his care and skill in getting these photographs. They are particularly comforting to me because, on my way home from the eclipse, I read in a halfpenny paper that the climate of Greenwich was so bad that observations could no longer be carried on there. But these photographs of the sixth and seventh satellites obtained at Greenwich show that we are still able to do a little there. (Cheers.) The seventh satellite is a very difficult object, as is well known; but still we have got photographs of it on five nights, and of the sixth satellite on quite a number of nights, about 19 up to the present time.

The picture now on the screen is a diagram showing the positions of Jupiter and of the sixth and seventh satellites on all the nights of observation. I call attention in passing to the fact that the exposure being so long the image of Jupiter itself is reversed photographically, and appears as a white disc from which measures can be made with considerable accuracy. The photographs, in fact, have been measured so far by using a position-angle micrometer. This diagram then shows the path of Jupiter in the sky to November 7, which is the date of the last photograph we have got, and also the tracks of the sixth and seventh satellites. The seventh satellite moved along a path which you see is quite different to that of the sixth. The dates of observation of the seventh satellite were October 22, 29, 31, November 3, 6, and 7, and it may be noted that on one of these nights — October 29 — the seventh satellite was shown on two photographs, and shown on one of them with an exposure of 17 minutes only. The exposures have usually been 3 hours down to 1½ hours. 1½ hours is usually ample.

This next diagram shows the relative movement of the two satellites round Jupiter, deduced from the photographs. It will be seen that a considerable part of the orbit is covered by these observations. The seventh satellite appears to be moving in a direct line towards Jupiter, It has now got so near Jupiter that it is doubtful whether we shall be able to get any more photographs until it comes out on the other side, because the satellite will be buried in the large diffraction-image round Jupiter. I should add that these photographs were taken with the 30-inch reflector made by Dr. Common.

[*The Astronomer Royal* then exhibited a table giving a comparison of the results of the measures with the ephemeris places, and showed copies of the actual photographs, described as follows:—]

On this photograph the sixth satellite is shown distinctly. It will be noted the stars have all trailed because the guiding was on

Jupiter. That star trail represents the motion of Jupiter, and the satellite is nearly round. There is only the orbital motion of the satellite round Jupiter in the interval. Here is the seventh satellite, which is quite distinct. I will hand round the lantern-slides, and it can be picked out with the help of the lens.

This had an exposure of 2 hours. Well, the interesting point about this is that, of course, the stars show long trails which correspond to the motion of Jupiter; but the satellites are picked out at once by showing a short trail in a different direction. That is the sixth satellite trailing in that way, and the seventh has a longish trail about 7 seconds in length in that direction.

This is the one taken with 17 minutes exposure. There is the seventh satellite. That photograph was really taken with the object of getting the sixth satellite, but we were fortunate on an exceptionally fine night to get the seventh as well.

Here is another one showing the seventh satellite there, and the sixth is quite striking.

Here is the sixth, quite a bright star, I think of the 14th magnitude, and this is the seventh, which is quite measurable.

This is the last, taken on November 7, of the sixth satellite; and here I should explain that in the later photographs the motion of the seventh satellite was allowed for in guiding; that is to say, the guiding was on Jupiter with an allowance for the motion, so that we get the satellite condensed in a point as nearly as may be; and in that way we were able to get a better photograph of it. These trails were taken with supplementary exposures in order to get the orientation. Another point: the measures which are here given are only to be considered as provisional. We propose to refer the positions of the satellites to stars in the field, and Mr. Dyson has suggested what I think is a very good plan for that: that is, to take supplementary photographs with the astrographic telescope, to measure in the first instance on the reflector photographs the position of the satellites from three or four neighbouring stars surrounding them, which will be equally affected by distortion of the reflector, or nearly so, and then refer these stars to well-determined reference-stars by means of photographs taken with the astrographic telescope, and reduced in the ordinary way....In that manner we hope to get great accuracy in the measurements.

Prof. Turner. I think what we have heard reflects great honour upon Greenwich Observatory generally, and on Mr. Melotte in particular. I should like to point that remark by saying that, after the first photograph had been taken on October 22, but before the second one, Dr. Perrine, the discoverer of the sixth and seventh satellites, on his way back from observing the eclipse, to my great delight came to stay with me at Oxford. He arrived on the 28th, having been to Greenwich the day before, and he told me he had seen with great interest the photographs of the sixth satellite, but he did not think it would be possible to get the seventh in our climate.

He knew from bitter experience the length of time required to photograph it at the Lick Observatory; and gave it as his opinion that it could not be got at Greenwich. Almost while he was speaking, I imagine, the second photograph, viz. that of October 29, which confirmed the first one, was being taken or about to be taken, and now, from the evidence we have had before us, we see the satellite has actually been photographed in so short a time as 17 minutes. No one, I think, will be more delighted to hear that than Dr. Perrine. I am sure the work done by large reflectors is of the very greatest interest, and will stimulate those who have them to further effort.

The President, in thanking the Astronomer Royal on behalf of the Meeting, said that it must afford great satisfaction to himself and his staff to be able to bring forward such evidence of the highest class of observational work being carried on even in the climate of London. There was, he added, another paper from Greenwich on Neptune; perhaps Sir William Christie would like to say something about that now.

The Astronomer Royal. This is a paper giving results of the measures of photographs of Neptune's satellite in continuation of those which have been published. The interest of the paper lies in the table, which shows discordance between the tabular and observed places of Neptune, and also incidentally the discordances between the results of different photographs on the same night—from which we get an idea of the accuracy in the measurements, which is very satisfactory. Mr. Dyson and Mr. Edney have already discussed some similar results, and these are in continuation. These photographs were obtained with the 26-inch refractor, using an occulting shutter. In the case of Jupiter the photographs were taken with the reflector, so that we had not an opportunity of using the shutter.

The President, after thanking the Astronomer Royal, called upon Father Cortie to show his slides relating to the eclipse.

Rev. A. L. Cortie described the scenes of his observations of the eclipse at Vinaroz, where he had for his companions Father Morford, of Saltash, Cornwall, and three of his (Father Cortie's) pupils, and fortunately had a clear sky. His instrument was a telescope of 20 feet focal length, working at *f*/6. He showed copies of his pictures of the corona and prominences on the screen, which were mostly taken on Imperial ordinary plates with exposures varying from 4 to 50 seconds. He pointed out dark rifts shown in his pictures of the corona, and remarked that, after careful examination, he thought that this was dark matter and not the effect of contrast.

Father Cortie also showed photographs taken by Mr. Benson, Mr. Dickson, and Rev. R. Killip, who were at Burgos, and by extraordinary good fortune were at a place where the clouds, which had previously covered the sky, cleared from before the Sun for 4 minutes, which included totality.

Dr. Spitta. There is a good deal of trick in getting lantern-slides. I think the great art is to develop exceedingly slowly, using plenty of bromide and going patiently on till you can get no more, meanwhile keeping the fog down by using bromide. If you only go on long enough you will get the streamers. And if the lantern-slides are made in a camera by a feeble light the streamers will be found further out than they will if the slides are made by actual contact.

The Astronomer Royal. I can corroborate the remarks of Dr. Spitta about using weak developer and developing slowly, and also about making copies of plates not by contact but in the camera. To observe this eclipse we (Mr. Dyson and I, with Mr. Davidson) went to Sfax, in Tunisia. Among our instruments we had the Thompson coronagraph of 9 inches aperture, which was used with a concave lens, forming, in fact, a telephoto combination and giving a 4-inch image of the Sun. Then we took out the astrographic object-glass of 13 inches aperture and 11 ft. 3 in. focal length, giving an image of $1\frac{1}{3}$ inch, our object in using that being to get greater extensions of the corona. We had also a Dallmeyer rapid rectilinear lens of 32 inches focus working at *f*/6, and a more rapid Unar lens working at *f*/5 of 12 inches focus, so that we hoped to get the detail of the inner corona and the prominences combined with great extension. But our intentions were to a certain extent frustrated as regards great extension by having a cloudy day for the day of the eclipse, the only day during a long time before and after that there had been clouds there. Still the clouds cleared away sufficiently about the time of totality to enable us to get very satisfactory results for all the inner parts of the corona, and considerable extension up to about 90′ from the Sun's limb. But no doubt if it had not been for the clouds we should have been able to get considerably greater extension of the fainter rays of the corona. I will run through the photographs rapidly.

The first one was taken with the Unar lens, and really the chief interest is in showing how the clouds hid the extension of the corona.

This, again, taken with the Unar lens, shows the clouds.

This was taken with the Thompson coronagraph. The noticeable features are the great prominences and disturbance of the corona all round, which is very remarkable.

This one was taken with the astrographic lens enlarged to the same scale and shows that the focus is extremely good, thanks to the care Mr. Davidson had taken in focussing. That shows a little more of the inner corona. There is a sort of oval ring here (in the corona) shown with a bright centre, which will be noticed in other photographs, and it will be seen arcs of other rings come out round it. There are four rings that can be traced; three outer ones.

The next shows greater extension of the rays. I think, with Father Cortie, that it is absorbing matter that causes that dark ray.

There is another dark ray here. The striking feature seems to be that some rays stand out in front of others. We see the corona in perspective. These form the background, and these others cut across them. This dark ray cuts across the rays in the background.

The Astronomer Royal continued to show photographs taken by the Royal Observatory expedition at Sfax, and added:—I would like just to make a remark about these peculiar features to which I have called attention, these oval rings with bright centres. They seem to me—these several rings or arcs—strongly suggestive of an explosion taking place; they look like rings high up in the atmosphere seen in perspective. Somewhat similar structures have been seen in other eclipses, but I think they have come out more strikingly in this corona, which seems to be in a more disturbed state than in any of which we have a photograph in the Greenwich series.

Mr. Thwaites. The photographs I have to show you were taken at Burgos. I only propose to show a few slides and to make a few remarks upon them, reserving details for a future occasion. A party of twenty was organized by the British Astronomical Association, and I had the honour of being the leader of the party. We went to Burgos, and there were very much aided by Señor Iniquez, Chief of the Madrid Observatory, who gave us a station about two miles out of the city, and afforded us immense help in every way. He gave us our latitude and longitude and Madrid time. I had intended to take a series of photographs on slow plates at the beginning and end of totality, and faster plates in the middle of the totality, but on the day of the eclipse the sky was completely overcast and I had to alter my plans, changing my slow plates for those of quicker speed within half an hour of totality. Up to within 15 seconds of totality none of our party had any hopes of seeing anything, when suddenly the clouds seemed to break up and a rift appeared, which, however, only lasted for five minutes. Two of the party even packed up their instruments and put them into the tent, thinking there was not the slightest chance of doing anything. However, they soon unpacked, and fortunately were able to take some photographs.

Mr. Thwaites showed and described his photographs of the corona.

Mr. E. W. Maunder then gave an account of the expedition made by himself and his wife to Labrador.

Following this, he exhibited some photographs of the sun-spots that were so conspicuous in the latter part of October. The first of these came on the Sun on October 14. The photograph showed the group as taken with the Dallmeyer photoheliograph when passing the central meridian on October 21. This group was not quite so large as the group which was on the Sun at the beginning of February, but it was certainly one of the largest groups of the the present cycle.

This group was followed by another, not quite so large and of very different character. This second group was also very easily visible to the naked eye. The photograph shown was taken on October 28, when the spot was just past the central meridian, and shows the general character of the spots very well.

The Astronomer Royal has desired me of late to try some experiments with the 26-inch refractor, and I have here two plates which give the Sun on a scale of 30 inches. The first shows the great spot taken just as it was passing off, and the other shows the second of these two great spots. The faculæ are brought out very fairly well. It was taken with a Thornton-Pickard focal plane shutter, which is supposed to give an exposure of 1-800th of a second. This last photograph was taken, again with the 26-inch telescope, of a smaller group that has recently come on the Sun, taken on Tuesday of this week. The detail seems to be brought out remarkably well in the penumbra and round the edge of the umbra. The fringe of the penumbra is very well seen on the northern edge of the umbra.

Mr. Thwaites. I should like to add that the instrument I used was a 4¼-inch photo visual telescope lent to me by Mr. Newbegin, being the same instrument I used in India in 1898.

Mr. S. A. Saunder. I have no photographs to show of the total eclipse, but I have a photograph of the partial eclipse taken by a pupil of Wellington College. He showed me the photograph at the beginning of the term, and it seemed to me so interesting that I showed it to Mr. Maunder. He has made a copy of it, and suggested that I should put it on the screen to-night. The point of it is the large amount of the Moon's disk that is shown. Besides the part of the lunar limb which is on the Sun, the remainder can be traced almost completely round the circle. It was taken through clouds by a school-boy with an ordinary camera.

Prof. H. H. Turner. I imagine that if some of us say little about these papers it is because we have so much to say rather than so little. But I should like to elicit from Prof. Fowler what I think he could tell us about the prominences to which the Astronomer Royal called attention.

Prof. Fowler. On the morning of the eclipse, after a shower of rain at the station where I was at Castellon, we had a perfectly clear sky, and I was able to observe the prominences round the limb, though afterwards the clouds came on. During this period before the eclipse I noticed particularly about the position of the prominence that the Astronomer Royal has called attention to, that there was a smaller prominence, which was remarkable for its great brightness. In this prominence I saw an immense number of bright lines of sodium, iron lines, and other substances. Again, after the eclipse we had the sky clear, but we had invited the people who cared to come to see the instruments, and there was no further opportunity of examining what had happened to any particular prominence. That was an observation I had not seen the necessity for when we invited the visitors.

Mr. W. W. Bryant. It may be of interest to mention that the magnets lately have been exceedingly quiet, though, as shown by the spots, the Sun has been greatly disturbed.

Mr. P. H. Cowell, after remarking that he had some eclipses to talk about, but had no pictures of them to show, because they occurred rather before the invention of the camera and the spectroscope, proceeded to explain his recent researches in the lunar theory, the substance of which appeared in this Magazine for November, and also described another paper on a similar subject which is summarized on page 454.

Prof. H. H. Turner. It is difficult to express too great an interest in the idea of this paper. May I give an illustration of what I take to be the situation from a different example? It seems to me that things are rather as they were some dozen years ago, when Prof. Kustner and Dr. Chandler thought that they had shown from observation that the latitude of places on the Earth was variable, and Prof. Chandler gave an explanation of this. When he was told that theoretically he must be wrong, he said so much the worse for the theory. Ultimately it was found there was a theoretical flaw. They had forgotten the assumption that had been made in assuming the Earth to be rigid. Now we seem to have come across a case similar. Mr. Cowell has found evidence from old eclipses that there was a certain motion which in the first instance was ascribed to the node, and he was told the theory would not admit of it; but on searching the theory it occurred to him that the forgotten assumption was that the Earth is going uniformly round the Sun, and he asks us to give up that assumption and to consider that the motion of the Earth is being resisted. The nature of the evidence is summed up, it seems to me, extremely well in the last sentence of the paper in the supplementary number which most of us, I think, only received this morning, so that Mr. Cowell must forgive us if we are not ready to digest all he has told us.

We cannot imagine that by accident the five eclipses mentioned in history in the way put down should be rendered total by some conjuring trick, and from that point of view I think we may say he has produced a true cause, or something equivalent. It is too early to say that is the correct explanation, but he has brought us face to face with a fundamental explanation of that kind. I do not know, perhaps he will tell us, how much revision of our notions of planetary motions this will mean. It may be that in the case of the other planets the motion may be insensible. I would like to congratulate him on the brilliance of this discovery.

Mr. Dyson. Prof. Turner has rather anticipated what I was going to say. It seems to me that the results are so accordant that one would have accepted without any question the value found by Mr. Cowell for the Moon's secular acceleration. That the Sun should have a secular acceleration is such a startling result that it is desirable to add any additional evidence if such can be found.

Mr. Cowell. The secular accelerations found from ancient solar eclipses of 2500 years ago have a weight proportional to the square of 25. It is difficult to check the conclusions from modern observations, which, roughly speaking, are confined to the centuries before and after 1800. To make the two classes of observation comparable, the precision of the modern observations would have to be greater in the proportion of 25 squared to one. This is not the case. The transits of Mercury do, however, afford evidence, though of little weight, that the Earth's motion is subject to a secular acceleration. It seems, however, that Mercury has no secular acceleration, and this is inconsistent with the only physical cause that I can at present suggest—the resistance of the æther.

Mr. Knobel called Mr. Cowell's attention to certain astronomical observations contained in Japanese records which might help him in his researches.

The following papers were announced and partly read:—

Royal Observatory, Greenwich. "Observations of the Satellite of Neptune from Photographs taken between 1904 Nov. 11 and 1905 April 15." (Communicated by the *Astronomer Royal.*)

P. H. Cowell. "On the Secular Acceleration of the Earth's Orbital Motion."

P. H. Cowell. "On the Ptolemaic Eclipses of the Moon recorded in the Almagest."

John Tebbutt. "Observations of Phenomena of Jupiter's Satellites at Windsor, New South Wales, in the Years 1900 and 1902."

E. Nevill. "On the Corrections to Hansen's 'Tables de la Lune,' as deduced by Mr. Cowell."

Royal Observatory, Greenwich. "Micrometric Measures of Double Stars made with the 28-inch Refractor in the Year 1904." (Communicated by the *Astronomer Royal.*)

Radcliffe Observatory, Oxford. "Observations of the Partial Eclipse of the Sun, 1905, August 29–30." (Communicated by the *Radcliffe Observer.*)

Royal Observatory, Greenwich. "Observations of Jupiter's 6th and 7th Satellites from Photographs taken with the 30-inch Reflector of the Thompson Equatorial." (Communicated by the *Astronomer Royal.*)

The following gentlemen were elected Fellows of the Society:—

Walter Sidney Adams, Solar Observatory, Mt. Wilson, California.

Ernest Percival Cotton, Lands and Survey Dept., Lagos, West Africa.

Rev. Alex. C. Henderson, B.D., The Manse, Delting, Brae, Shetland.

Rev. Fredk. Jervis-Smith, M.A., F.R.S., M.Inst.E.E., Trinity College, Oxford.

T. Hobart Pritchard, 5 Cotford Road, Thornton Heath, Surrey.

The following Candidates were proposed for election as Fellows of the Society:—

Lieut. Geo. Alfred Alcock, R.N.R. (proposed by W. M. C. Hepworth).

Hugh Danl. Badcock, M.A., Assoc.M.Inst.C.E., Town Engineer, Pretoria, Transvaal, S. Africa (proposed by R. T. A. Innes).

Rev. Arthur Edwin Brisco-Owen, M.A., The Rectory, Cholderton, Salisbury (proposed by Rev. C. D. P. Davies).

Wm. Robert Bruce, Observatorio Magnetico, Pilar, Provincia de Córdoba, Argentine Republic (proposed by R. Copeland).

Lieut. Herbert Archer Edwards, R.N.R., Marine Office, Lagos, W. Africa (proposed by Thos. Lewis).

Crawford M. Fleming, 14 North Kinver Road, Sydenham, S.W. (proposed by Rev. D. Fleming).

Edgar Geil, M.A., F.R.G.S., Doylestown, Pennsylvania, U.S.A. (proposed by E. A. Reeves).

Rev. Thos. Henry Leale, A.K.C., 65 Gaskarth Road, Balham Hill, S.W. (proposed by Rev. F. B. Allison).

Lieut. Fredk. Wm. Mace, R.N.R., 2 Rutland Avenue, Sefton Park, Liverpool (proposed by Thos. Lewis).

William Ottway, Orion Works, Ealing, W. (proposed by A. Fowler).

Legh Richmond Powell, 10 Cranfield Road, Bexhill-on-Sea, Sussex (proposed by Rev. F. B. Allison).

Rev. John Gardham Reed, Burlington Manse, South Road, Saffron Waldon, Essex (proposed by Rev. Wm. Smith).

Richd. Frind Roberts, Westcroft, Worlingham, Surrey (proposed by John Evershed).

ANNUAL GENERAL MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905, October 25.

A. C. D. Crommelin, B.A., *President*, in the Chair.

Secretary: J. G. Petrie.

Mr. Petrie read the Minutes of the last Annual General Meeting. He next read the list of presents received, and the thanks of the Association were accorded to the respective donors.

The names of 16 Candidates for Membership were read and passed for suspension, and the election by the Council of 10 new Members was unanimously confirmed.

The President said a large number of reports with reference to the recent eclipse had been received and there were others still to come in. There was, as a rule, not very much time available for the reading of papers at the Annual Meeting, and the Council had therefore decided that the next Meeting of the Association, to be held on November 29, should be devoted chiefly to the consideration of reports on the eclipse.

Mr. Petrie read the report of the Scrutineers of the Ballot for the election of Officers and Members of the Council for the Session 1905–06. The following were declared duly elected :—*President*: A. C. D. Crommelin. *Vice-Presidents*: E. W. Maunder, S. A. Saunder, W. H. Wesley, C. T. Whitmell. *Treasurer*: W. H. Maw. *Editor and Librarian*: F. W. Levander. *Secretaries*: J. G. Petrie, J. A. Hardcastle. *Other Members of Council*: G. F. Chambers, Tyson Crawford, W. Heath, H. P. Hollis, W. T. Lynn, Mrs. E. W. Maunder, G. J. Newbegin, D. Smart, T. Thorp, A. Stanley Williams.

The following nomination of Members by the Council as Editor and Directors of the Observing Sections was unanimously confirmed :—*Editor*, F. W. Levander. Directors of Observing Sections : *Sun*, Rev. A. L. Cortie ; *Moon*, W. Goodacre ; *Mercury and Venus*, Henry MacEwen ; *Mars*, E. M. Antoniadi ; *Jupiter*, Rev. T. E. R. Phillips ; *Saturn*, G. M. Seabroke ; *Comets*, E. W. Maunder ; *Meteors* (vacant)* ; *Star Colours*, E. I. Essam ; *Variable Stars*, Col. E. E. Markwick, C.B. ; *Double Stars*, G. M. Seabroke ; *Photography*, R. Wilding.

The adoption of the Report of the Council on the work of the Session, October 1, 1904, to September 30, 1905, together with the accounts, was carried unanimously, as were votes of thanks to the retiring Members of the Council, the Auditors, and the Scrutineers of the Ballot.

The President in beginning his address congratulated the Association on the signs of health and activity which it continued to show at the beginning of its 16th year of existence. It was true that their numbers had declined from the maximum of five years ago, but they still had a membership of 1000, which was held out by their first President, Capt. Noble, as an ideal to be aimed at, and which was probably the highest figure they could hope to maintain permanently. Their principal Observing Sections appeared to be in a condition of full vigour, though in some of them an increase in number of workers would be very welcome. There were two Sections in particular—the Variable Star Section, in which much useful work could be done with a good binocular, and the Meteoric Section, which needed only a star atlas and a knowledge of the Constellations.

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* Miss C. O. Stevens has now been appointed.

The great eclipse of 1905 had come and gone, and once more the Association had evinced its usefulness in encouraging a spirit of comradeship and *esprit de corps* among the observers. He thought that, on the whole, they might congratulate themselves on the result of the late eclipse, and their sincere sympathy would be extended to Mr. and Mrs. Maunder and Mr. Evershed in their failure to obtain results.

The President then proceeded to pass in review some recent advances in astronomy, among which he considered that the photographs of the Martian Canals secured at the Lowell Observatory took a high place. The late Mr. Green, in his Presidential Address in 1897, predicted with great acuteness this result. The recent 'Bulletin' that described Mr. Lampland's success stated that the difficulty of atmospheric blurring was overcome, as Mr. Green had suggested, by taking a large number of exposures with an apparatus somewhat resembling the bioscope, but permitting exposures of eight seconds or thereabouts. The results were a very notable advance in planetary photography. The remarkable discoveries in the satellite world had produced quite a revolution in many of their ideas, and Dr. Perrine was entitled to their heartiest congratulations on his most interesting discoveries, which were in no sense accidental, but the well-earned reward of a deliberate search.

Another point on which he would like to say a word was the great attention that had been given in recent years to the study of motion in the line of sight by the spectroscopic method. It had been applied to the solar system and the stellar heavens: attempts had been made to obtain by its aid the rotation periods of the Sun, Venus, and Uranus, though it must be confessed that the problem was such a delicate one, from the very slow velocities in question, that they could not give very much weight to the results obtained; thus M. Belopolsky found a short period for the rotation of Venus, while Prof. Lowell's results favoured a period of 225 days. But in the stellar heavens materials were being rapidly accumulated, which would in a few years place their knowledge of the structure and motion of the sidereal system on a much firmer basis, and enable them to deduce the mean distances of stars of various magnitudes by comparing their linear velocity with their apparent proper motion. The spectroscope had also revealed to them the existence of a great number of most interesting binaries, with periods of only a few days, and had enabled them to measure the distance from us of a few binaries whose period was short and orbit well determined.

In this brief review of current work he had selected those points in which he was personally most interested; he felt justified in adopting that course, as he was following the example set by many of his predecessors in that Chair. He concluded by wishing continued and ever-increasing prosperity and efficiency to the Association, and hoping that all the Members would do as much as lay in their power to promote those ends, which they could do (1) by observing; (2) by writing papers; (3) by joining in the discussions at their Meetings; or (4) by endeavouring to spread a zeal for astronomy among their friends, and to persuade them to enlist themselves in their ranks.

Mr. S. A. Saunder was sure all would agree with him when he said they had listened to an exceedingly instructive and interesting address. They would all, he was sure, wish to express their most hearty thanks to the President for what he had done for the Association.

Mr. W. T. Lynn seconded the vote of thanks, which was carried with acclamation.

Mr. Levander read a paper by *Mr. H. Thomson* on "An Instrument for determining the Central Meridian of Jupiter" and another by *Mr. Scriven Bolton* on "The South Tropical Markings of Jupiter."

Mr. W. H. Maw said the object which was desired to attain by the instrument described in Mr. Harold Thomson's paper could, he thought, be secured in a more simple way by the use of an ordinary micrometer or an eyepiece provided with two fixed webs only, these webs being fixed at a distance apart, either slightly less or slightly greater than half the diameter of Jupiter's disc.

Mr. G. J. Newbegin showed some photographs that he had taken of the great group of spots which were at the moment still to be seen on the Sun.

Mr. E. W. Maunder said Mr. Newbegin was certainly to be congratulated very heartily upon the beauty of his photographs, upon the enlarged scale on which they were taken, and upon the amount of detail and definition which the spot displayed as shown on his photographs.

Some drawings of the corona of the recent eclipse by *Miss Stevens* and *Miss Hart Davis* were handed round.

Mr. Gavin Burns, *Mr. S. Fellows*, and *Mr. Ashton C. Allen* exhibited photographs taken in connection with the recent eclipse.

The President said the eclipse reports and photographs would, as he had already announced, be discussed at the next Meeting. The Council, he added, had decided on this occasion to embody the reports of the eclipse in a special number of the "Journal." All who had not yet sent in their reports were urged to do so as soon as possible, for inclusion in that number.

The following papers were also announced:—"Interim Report, No. 11, of the Variable Star Section," by *Col. Markwick, C.B.*; "Observations of Vesta," by *R. F. Rendell*; "The Lunar Eclipse of 15 Aug. 1905," by *Prof. Moye*; and "The Green Flash," by *C. T. Whitmell.*

The Meeting adjourned till November 29.

ROYAL METEOROLOGICAL SOCIETY.

The opening Meeting of this Society for the present Session was held on Wednesday evening, November 15, at the Institution of Civil Engineers, Westminster: *Mr. Richard Bentley*, President, in the Chair.

Sir John W. Moore, M.D., communicated an interesting paper on "The Rainstorm of August 24th to 26th, 1905, in Counties Dublin and Wicklow," which, in the absence of the author, was read by the *Secretary*. The atmospheric disturbance which caused the torrential rainfall was near the shores of Kerry and Cornwall on the 24th, and the next morning it was near the Scilly Islands. Thence it travelled slowly northwards up St. George's Channel, its centre passing near Dublin early on the morning of the 26th. At this time the system suddenly changed its course, crossing the channel eastwards to Wales, and finally passing over central England and out to sea at the mouth of the Humber in a north-easterly direction. It appears that the rainfall on the 25th exceeded three inches at all stations in the Counties Dublin and Wicklow, while it rose above four and even five inches at stations near the Dublin and Wicklow Mountains.

Sir John Moore is of opinion that this remarkable downpour was brought about by the co-operation of the following factors:—(1) A chill antecedent to the arrival of the rain-bearing depression; (2) the slow progress of the depression; (3) the fact that the Counties Wicklow and Dublin lay to the westward of the cyclonic centre, and so received its north-easterly and northerly winds; and (4) the physical configuration of those counties and their coast-line. As the result of this remarkable rainstorm, a destructive flood occurred over the low-lying parts of the Bray Urban District near the mouth of the Bray River. At Little Bray the water rose to a height of 4 feet in the streets, flooding houses, destroying domestic animals and fowls, wrecking furniture, and covering floors, yards, and gardens with a thick alluvial deposit.

A paper by *Dr. W. B. Newton* was also read, describing "The Aquameter." This is a new instrument for accurately measuring the amount of aqueous vapour present in the atmosphere.

On the Secular Acceleration of the Earth's Orbital Motion.

In this article I endeavour to describe as simply as possible the chain of reasoning by which the secular acceleration of the Earth's orbital motion was inferred. The calculations are given in the last two numbers of the 'Monthly Notices': the principles are alone described here.

From records of eclipses the relative positions of three points are inferred—the Moon's node, the Moon, and the Sun. The geometry is more simple for a lunar eclipse than for a solar eclipse. For a lunar eclipse it is evident that the time of the eclipse depends upon the distance between the Sun and Moon at a definite instant near the eclipse. If, for instance, at the definite instant, the shadow of the Earth is one degree in advance of the Moon, we may infer that the middle of the eclipse will occur in two hours, if we take twelve degrees a day to be the rate at which the Moon gains upon the Sun. Again, the distance between the node and the Sun determines the magnitude of the eclipse, for it determines the latitude of the Moon when in opposition to the Sun.

For a solar eclipse the geometry is more complicated, but the geometry depends, in the main, upon the same three points as before. The distance between the Sun and the Moon at a given instant determines the time of conjunction, and therefore the face of the Earth turned towards the Sun at conjunction. The distance between the Sun and the node determines the latitude of the Moon at conjunction, and hence determines how far north or south the axis of shadow falls upon the Earth. Conversely, if the time of a lunar eclipse be observed, we have a record giving the distance between the Sun and Moon at a given instant; if the magnitude be observed, we know the distance between the Sun and node at a given instant. If both be observed, we can determine, for a given instant, the relative positions of the three points, node, Moon, and Sun.

Again, if we have the single fact that there was a total solar eclipse at a given place, we can get a single equation of condition connecting the distances of the Sun from the node and from the Moon.

Let us suppose the central line of an eclipse runs E.S.E., and let us suppose that a total eclipse is recorded a little to the north of the line, which, according to the tables, was the central line. Then, roughly speaking, if we alter the tabular distance between the Sun and the Moon, we can move the whole tabular central line eastwards; if we alter the distance between the Sun and the node, we can move the whole central line northwards; by either process, or by a combination of the two processes, we may bring the central line over the place where totality was observed.

One most important point is to be noticed. Although we can determine the differences of longitude between the Sun, Moon, and node, we cannot determine absolute longitudes—that is to say, we cannot determine where the equinox is relatively to the three points whose positions we may suppose ourselves to have found.

The position of the equinox, in fact, has nothing to do with a lunar eclipse. For a solar eclipse, let us suppose an observer on the Moon trying to sketch the geographical features of the Earth to compare it with tables supposed to predict the face of the Earth turned to the Moon at any instant. It is clear that such tables would presuppose a knowledge of the Earth's pole and equator and therefore of the equinox, but it is also clear that an error in the position of the equinox amounting to a degree would, at the outside, only cause an error of one minute in the position of any place on the Earth as seen from the Moon. The tabular position of the equinox cannot therefore be corrected from a solar eclipse, although we cannot say, as in the case of a lunar eclipse, that it has nothing to do with it.

Another point to be noted is the extreme precision of a solar eclipse, considered as an astronomical observation, as compared with a lunar eclipse. The value of a lunar eclipse depends upon the time and magnitude. The time observations of the early lunar eclipses are supposed to be subject to a probable error of 20 minutes, corresponding to 600″ in the distance between the Sun and the Moon: the magnitudes appear to have a probable error of about four per cent. of the Moon's diameter, corresponding to 900″ in the distance between the Sun and the node. For a solar eclipse, on the other hand, we require no measurement on the part of the original observer; a statement that he saw the corona (fire in the midst of heaven) or that stars appeared suffices. Even less than this will suffice if we have more than two eclipses, whose testimony agrees; for then, if the eclipses were not total, a most remarkable coincidence is left without explanation.

I now come to describing the argument by which the place of the equinox relatively to the node is assigned. During the last 150 years the motion of the node has been observed, and, if it differs from the theoretical value at all, the difference is negligible for the present purpose. The theoretical motion varies with changes in the eccentricity of the Earth's orbit, but during the last 150 years these changes have also been found to correspond with theory. Hence, although, in many cases in gravitational astronomy, it is necessary to admit that theory is imperfect, still in the case of the motion of the Moon's node and the change of the solar eccentricity it seems to be sufficiently accurate for the purpose. Hence we may fairly attribute to the distance between the equinox and the node its theoretical value. To upset the conclusion that follows upon this, if the evidence of the eclipses be considered decisive upon the relative configuration of the Sun, Moon, and node, it will be necessary to suppose that the node was 2500″ out of its theoretical place 25 centuries ago; or, in other words, that some unknown cause exists that during the last 150 years has produced no effect, but which upon the average, during 25 centuries, has moved the node 100″ a century.

Finally, partly by theory and partly by eclipses, we have the relative positions of the four points, equinox, node, Moon, and Sun, and the conclusion that follows is that the Sun's motion is not uniform, but is subject to an acceleration of 4″ a century, according to the usual definition of secular acceleration, but which really means that the motion in one century exceeds by 8″ the motion in the previous century. P. H. Cowell.

The Astronomical Society of the Atlantic at the Cape.

THE sudden change from apparent time at ship to Standard Time of South Africa, which is three-quarters of an hour fast on local time, put forward the dressing-bugle and brought us all on deck while it was still dark, to see what is certainly one of the finest sights in the world—Cape Town at night, from Table Bay. The mountain rises like a wall behind the lines of lighted streets, and the Southern Cross just above the Devil's Peak introduces us at once to one of the standing difficulties of Cape Town life, to make any sense of the geography and the ways of the compass. It is necessary to kick oneself continually to remember that Table Mountain is south of Cape Town, and that the Sun rises behind the distant Hottentot Holland mountains because they are in the east, not because sunrise in the Southern Hemisphere takes place in the west, as it seems to do—especially in Cape Town. Orion stands on his head beside Venus to herald the rising of the Sun, which *came up* with a green flash on that eventful evening, as one of the members from Cambridge observed.

At daybreak the ship is docked, and almost the first man on board is the Chief Assistant at the Royal Observatory, closely followed by His Majesty's Astronomer, to give the Society the heartiest kind of welcome, and invite a good part of it to stay at the Observatory itself, while the rest find themselves received with equal hospitality by good citizens of Cape Town, who declare that the one thing that pleases them is that their guests have come a whole week early.

We knew that we should find the Royal Observatory at the Cape one of the best equipped and most interesting observatories in the world; but we were not warned that it is also one of the most charmingly situated. The stone-pines and cactus, the mimosas, the carpet of many-tinted oxalis, and the distant blue mountains are not equalled on the Riviera coast, and no mountain in the world ever looked finer than did Table Mountain on the magnificent early spring morning when we first made its acquaintance. "I would take some seeds of that mountain and plant them in Holland" was the opinion of one of our members as we enjoyed the view from Mr. Hough's front door, and some of us resolved that if he succeeded we would barter Madingley Hill for a seedling. There are the most fascinating weaver-birds building close to the Director's house, and blue blackbirds with brown wings by the Chief Assistant's. And the night is made glad by the song of the "Cape nightingales" in the water-meadows full of arums, or pig-lilies, over on the side where the base cavalry camp was pitched six years ago. There are three parts in this bull-frogs' chorus, explained by an old resident as representing the rattle of musketry, the racket of pom-poms, and the boom of the

Long Tom respectively. And last, but not least, of the amenities of this delightful place, there are two magnificent Kroomen in naval uniform from the dockyard at Simon's Town. Krooman Prince of Wales is no longer of the Observatory staff; but Peter and King Solomon shoulder our heaviest trunks, with beaming smiles on their coal-black faces.

Our first enquiries are naturally for the CLOCK, whose fame is already dear to everyone in Cape Town who knows the Royal Observatory. It has, we learn, played positively its last possible trick, and will be performing perfectly in time for the British Association Meeting. And so it was. Installed in the old magnet-house, with its solitary nickel-steel pendulum swinging in an inner shrine kept automatically at a constant temperature and low pressure, with its heavily built "slave clock" outside, released every second and made to do all the work, it is indeed a wonderful piece of mechanism. There were people at one time who said there was a deal too much electricity about it, and we gather that it has not been easy to get rid of the sparking troubles which generally make mischief with electric clocks. But these troubles have at last been overcome, and the only thing that raises a doubt whether the clock will ever beat the Earth as a time-keeper is the suspicion that the round steel-wires which form the elegant geometrical suspension of the pendulum may undergo a secular flattening and let down the pendulum gradually *. It remains to be seen whether this cause will or will not operate upon the pendulum's time-keeping more detrimentally than do the net effect of meteoric accretions and shrinkage of the whole mass upon the time-keeping of the Earth.

An even more beautiful example of electrical-temperature control is to be seen working perfectly on the large spectrograph of the McClean telescope. In a little room off the dome is a small piece of apparatus which is the most diabolically ingenious thing with which the writer is acquainted. Every half minute or so a lever tries to pass down between two jaws under which a galvanometer-boom is hanging. When the temperature falls a fraction of a degree in the spectrograph on the telescope the boom is deflected, and the lever passing down sends a current for a few seconds through the heating-coils of the prism-box. If next time the lever comes down the thing wants heat again, the heat is given it for a little longer time; if no more is wanted that time, and the next, the possible heating-period is successively curtailed, so that next time a dose is given at all it will be a smaller dose. In this way the temperature-control can not only give a dose of heat whenever it is required, but to a great extent can predict what size dose will

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* The suspension in question consists of a steel cylindrical bar (into the middle point of which the pendulum-rod is fixed) hung from an exactly similar bar, fixed horizontal, by two equal figure-of-eight loops of steel wire, one at each end of the bar.

be wanted. This goes on night and day continually, whether the spectrograph is mounted on the telescope, or removed to its stand to make way for objective-prism or direct photographic work; and at any moment an exposure may be begun with the certainty that the prisms are at the right temperature and will stay there. A Callendar recorder on the wall continually draws the temperature-curve of the prism-box, and shows that the temperature is constant within one or two tenths of a degree. How much labour was involved in making the whole thing work is probably known only to Mr. Lunt. But as it stands now, apparently working to perfection, it is perhaps the most beautiful thing ever sent out by Mr. Horace Darwin from the shops of the Cambridge Instrument Company.

To an astronomer accustomed to do a certain number of things for himself it is a little bewildering at first to sit at night in the dome of the McClean telescope and hear something going off automatically every minute or two. One is more or less used to the noise of the automatic-clock control, though one may be pardoned for wondering whether the new form of Grubb control, which makes a loud bang every second, is not a disimprovement on the older and more silent form. When to this perpetual racket is added the din of the non-silent clock-winding going off every four minutes, the whirr of the motors that revolve the dome, and of the smaller motors that work the quick and slow motions of the telescope, the sighing of the rams that raise the floor, the distant boom of the pump that supplies them with water, and the noise of the automatic starting-gear that controls the pump, one begins to wonder whether the engine-room of a liner is not a relatively quiet place in which to make refined observations. The answer doubtless is that the modern astronomer may become as deaf as he pleases, since all his time observations can be recorded on the slow-running or the fast-running chronograph a quarter of a mile away if a thumping sounder on the wall of the dome is not good enough for him. But the quiet watches of the night are become an empty phrase.

In the laboratory of the dome there is a grand show of photographs—Phoebe and the satellites of Uranus; the looped nebula in the Greater Magellanic Cloud, which is one of the most beautiful things in the sky; some magnificent enlargements of ω Centauri, showing Bailey's variable stars, and some that are not Bailey's, but belong to the Cape; and a whole series of remarkable spectra taken with the objective prism. Some of these southern fields a degree square contain four or five extraordinary bright-line spectra, which suggests the curious reflection that we shall very soon, thanks to the big objective prisms at Arequipa and the Cape, know all about the remarkable spectra of the Southern Hemisphere, and practically very little about those of the Northern.

Perhaps this will counterbalance the fact that we have plenty of photographs of spiral nebulæ photographed in the Northern Hemisphere, and not a single one from the South.

And now for a visit to the new meridian circle.

At last Sir David's ambition is satisfied, and he finds himself in possession of a meridian circle which fulfils every condition that could be formulated as desirable in such an instrument. Its house is of steel, symmetrically shaped, as to its upper parts, about the axis of rotation. The house divides completely along the meridian, and the two halves are rolled apart by a motor. The floor is of steel, with free circulation of air underneath. The piers are of steel and full of water; they are heavily cased in non-conducting material and covered in polished mahogany. And all the essential parts of the instrument are also of steel, but sheathed in a copper tube which carries all the subsidiary gear. We had the pleasure of seeing, most of us for the first time, the registering transit-micrometer of Repsold, which has already proved its worth at Koenigsberg and elsewhere. And we saw also the new driving mechanism for the transit-wire, which is, we believe, a machine of Sir David's own devising. The process of taking a transit is now as follows:—Set a friction-wheel which slides on its axis to a scale of cosines of declination, and throw it into contact with a cone which can be driven uniformly. Start the motor and let it get up speed. When the star comes into the field, clamp the cone apparatus to the motor, by a switch at the eyepiece, and the motion is transmitted from the cone through the friction-wheel and down to the transit-wire, which begins immediately to move with the star. Set it on the star by turning a milled head in the place of the ordinary R.A. micrometer, and give occasional touches to this head during the transit if the speed of the driving is not exactly perfect. Signals are sent to the chronograph at the instants when the moving wire is coincident with the imaginary fixed wires of the field, but the observer is not conscious of these, and devotes all his attention to keeping the star-image always bisected. Of course the eyepiece travels with the wire, and by this time it is no doubt fitted with a reversing-prism that is thrown round automatically when half the transit is complete. At the end of the transit the motor is switched off, and also the tape chronograph, which is in a separate house.

It is a pleasant thing to learn an entirely new way of transit-observing, as different as possible from the old. But to the novice it rather suggests the description of motor bicycling, as "all very well when you have got used to fighting with the taps." And the novice can do one thing which he could not do by the old methods—he can record an absolutely bogus transit not to be distinguished on the chronograph record from a genuine observation, by just letting the machine run without getting it on a star at all. No doubt there is some way in which the observer can record on the tape when a record is bogus—when a cloud comes up just at the critical moment, for instance—but we forgot to learn it.

A wonderful lot of good notions are embodied in the details of the instrument. The "wiring" of all the lamps for the microscopes consists of narrow brass strips screwed to the mahogany case, so that it is all entirely neat, self-explanatory, and accessible. The circles are lit by red lamps instead of white, which is very pleasant. The platform from which the nadir-readings are made is fixed on the top of the heavy reversing-carriage, and feels in consequence most unusually safe. But the most comfortable arrangement of all has a thoroughly naval appearance—hemp ropes fixed in iron stanchions along each side of the track of the observing-chair, so that the observer can pull himself in and out without the usual struggle to get hold of something other than the piers of the instrument. One needs but a single trip on the observing-chair to appreciate the value of this idea.

The collimators and azimuth-marks of the new instrument require a whole chapter to do justice to them. For two or three years references have been made in the Annual Reports from the Cape of the difficulty of obtaining good foundations for azimuth-marks, which led to the adoption of a remarkable expedient. Truth is now found at the bottom of various wells carried down to the solid rock, upon which there rests in each well a basin of mercury and an objective. The centres of these objectives are the points upon whose fixity everything depends, and it is hard to see what can possibly shift them. The marks upon which the instrument is pointed are adjusted vertically over these sunken marks by a simple optical arrangement whenever they are to be used. The shafts are lined with cast iron, and lighted and ventilated thoroughly, so that everything is in favour of the absolute permanence of the bottom marks. It will be disappointing if we do not hear soon that the azimuth component associated with the variation of latitude has been detected.

The unceasing attraction of all these new instruments did not prevent us from paying homage to the old, which have a great record to their credit. We made a reverential visit to the 7-inch heliometer, which is now busy observing the planets at opposition, referring their places to the comparison stars which have been determined lately at very many observatories. It was interesting to hear that the motion of one at least is better represented by Leverrier's tables than by Newcomb's.

An interesting point was noticed on a visit to the Astrographic equatorial. The driving-clock is controlled, not by a pendulum in the dome, which can be rated at pleasure, but by the standard sidereal clock. At first sight this looks curious, because, owing to the variable rate of change of refraction, a somewhat large and variable losing rate has usually to be given to the control pendulum. It would have been instructive to find out exactly why the perfectly constant rate suits this particular case. Is the polar axis so placed that the instrumental rate due to maladjustment balances the rate of the star due to change of refraction, so long as the exposure is made in the Cape zone and near the meridian? The success of such expedients, when work is restricted to a narrow zone, is very striking to one who photographs over a great range of declination.

The measuring-machine made to Sir David Gill's design by Messrs. Repsold has been such a success in the Astrographic work, that a duplicate is now installed. Those who read the account of this machine in the 'Monthly Notices' will remember that it demands great nicety of adjustment of the double spider-webs, which must exactly fit the réseau square. The way in which Messrs. Repsold have packed all these slipping-pieces and adjusting-screws into one small micrometer-box is quite marvellous. Of course one enquired about wear of the screws, for that was the objection to the machine that was raised when it was first described, in the same number of the 'Monthly Notices'* where it was proved that the screws of the Cape meridian circle wore out very fast. The screws of this machine wear out very fast also, but so nearly symmetrically with respect to the square of webs that almost identically the same error enters into the reversed measures. Consequently the mean result from the two positions of the plate is nearly free from screw-errors. This seems to be a beautiful example of how an apparently grave criticism may be really beside the mark. For astrographic work the machine is evidently a huge success; it only requires duplicating after the fashion of the Greenwich duplex micrometer to be quite ideal for the purpose. But for parallax work? This is another matter.

The sight of all this splendid new equipment of the Royal Observatory at the Cape gives rise to just one regret, that Sir David Gill has said that he will soon be coming home, before the years of anxiety which these new instruments have cost have borne a full crop of fruit. He is compelled to leave to someone else the very heavy responsibility of turning to its best use a wonderfully complex piece of machinery, just made to work. Not only our Society, who spent that fascinating week at the Observatory with him, but the whole astronomical world will wish him the delight of handling many volumes of Cape Annals and Catalogues filled with the output of the new equipment.

A. R. H.

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* Vol. lxi. pp. 61–76 and 135–139.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Prof. Lebon on the History of Hypotheses on the Nature of Solar Spots.

GENTLEMEN,—

M. Ernest Lebon, F.R.A.S., Professeur agrégé de Mathématiques au Lycée Charlemagne, has kindly sent me a memoir on this subject, which was communicated by him to the *Comptes Rendus* of the International Congress of Philosophy held at Geneva in September last year, with a request that I would communicate its substance to you.

He remarks that, between the discovery of solar spots and the time of Wilson, six hypotheses respecting their nature have been proposed and developed, maintained or contested. With regard to the history of most of these, so many errors have been current that it seemed desirable to go over them seriatim and correct, so far as possible, the erroneous points. From these are excepted the view of Scheiner that the spots are wandering stars passing over the solar disc, and that of Galileo that they are of the nature of clouds or smoke moving over the Sun as it rotates. This latter hypothesis has been held even in our own time by the famous physicist G. Kirchhoff, and is expressed in his 'Untersuchungen über Sonnenspectrum,' published in 1861. (If I remember right, Le Verrier maintained the same view.) Besides these two, four other hypotheses have been put forth, tabulated by M. Lebon as those of volcanoes, froths, scoriæ, and rocks.

The volcano hypothesis has been by several writers attributed to Cassini I., notably in the 'Journal des Sçavans' in 1688 in an article in which this view is developed at some length. Lalande, however, oddly enough, ascribes it to Derham, overlooking the fact that Derham himself attributed it to Crabtree* on the strength of a letter addressed by the latter to Gascoigne in 1640. It is really, however, due to Willebrord Snell (whose name is so well known as the discoverer of the law of refraction) and is put forth by him in his 'Descriptio Cometæ qui anno 1618 mense Novembri primùm effulsit,' published at Leiden in 1619.

The theory that the solar spots are of the nature of froth or foam was started by Descartes, but though later authors, Hevelius and others, have developed this idea, the priority of Descartes has been generally overlooked; hence M. Lebon quotes a passage in his 'Principia Philosophiæ' (p. 137, xciv), published in 1644, where it is clearly put forth.

Lalande rightly attributes to La Hire the view that the spots are, in fact, scoriæ, but is a little in error with regard to the date. For though La Hire slightly modified, in 1700 and 1702, his earlier ideas expressed in the 'Journal des Sçavans' in 1686, the main point is the same, founded on the view that "le corps du Soleil soit une matière fluide qui renferme en dedans des corps d'une autre matière solide qui ne puisse souffrir aucune altération et des figures différentes et fort irrégulières qui nagent dans la matière fluide du Soleil"; then, supposing that these small bodies are moved round the Sun's surface and thrown more or less above it, ... "il est évident qu'ils pourront nous faire voir les différentes apparences des taches du Soleil."

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* The only error I have noticed in M. Lebon's paper is that he spells the name of Horrox's friend as Crabtrie.

Finally, we have the rock-hypothesis, consisting in the theory that the Sun is an igneous sea usually covering asperities which, when this sea is agitated, become apparent and form the so-called spots. The earliest known mention of this is by Caramuel in the 'Mathesis Nova' *, published in 1670; but the expression used by him in the beginning of his reference to the theory, "Alii materiam densam et liquidam in Sole reperiunt," shows that others had suggested it before him. Yet Lalande, in his 'Astronomie,' speaks of it as "mon hypothèse." In a later edition he appears to prefer the view of La Hire; but he really seems to consider the rock-theory as his own, which is particularly strange because it is expounded by De Lisle as early as 1719 (as shown by M. Lebon from a manuscript which he recently presented to the library of the Paris Observatory) and published by Cassini II. in 1740. From some manuscripts by the latter it is evident that the rock-theory of solar spots was looked upon with favour by Cassini I. when he went to the Paris Observatory in 1671; but that he could not have been the originator of it is proved by the passage quoted above from Caramuel. After a very laborious search M. Lebon has hitherto failed to find the names of any of the "alii" who held it before him. So little is generally known of Caramuel that I may perhaps give here a few particulars about him. Born at Madrid in 1606, he became an ecclesiastic and held a great number of abbacies and other appointments in the Church, the last of which was that of the bishopric of Vigevano in Lombardy, where he died in 1682. He was a many-sided man, possessed great abilities as an engineer, and took part in the military defence of Prague against the Swedes in 1648. His works were on a great variety of subjects; perhaps we may sum them up in the language of Larousse, that they "portent l'empreinte d'une érudition solide, mais d'une imagination peu réglée et souvent bizarre." Yours faithfully,

Blackheath, 1905, Nov. 1. W. T. LYNN.

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* The full title of this work is 'Mathesis biceps: vetus et nova.'

The Obscuration of "Bridges" in Sun-spots.

GENTLEMEN,—

In the August number of your valuable journal, which has just reached me, I read a letter from Mr. W. H. Maw on the obscuration of "bridges" in sun-spots. As I have carefully studied the splendid sun-spot referred to, paid special attention to the "bridge" in question, and, besides my own memory, I have at hand daily photographs and drawings, I think I am in a position to answer the question of Mr. Maw.

I have sketched and photographed the said "bridge" in four conditions.

On the morning of the 13th of July at 8 A.M. (our Observatory is very nearly 8 hours E. of Greenwich) the bridge was very bright, uninterrupted, with sharp edges, resting easterly on the black background, and westerly on a half luminous granulated substance. The photograph shows only a bright line crossing a dark nucleus. A second photograph made at 4 P.M. on the same day shows the same bridge already cut off in its south-east part.

On the morning of the next day on looking at this object I was struck with the "brilliantly illuminated filaments" observed by Mr. Maw. But the bridge appeared to me as resting on a half dark jetty running between two dark places. It was tracing an elegant curve, brilliant in its northern part, but faint or rather interrupted in its south-east end. I find it so represented on my drawing as well as on the photograph. My impression at the time was that the "bridge" was fainting away and sliding through its brilliant filaments into the dark background of the nucleus.

On the next day, 15th of July, at 8 A.M., I saw at the same place a very different "bridge." The shape was quite different, and the edges did not show the brilliantly illuminated filaments of the previous day. Drawing and photograph agree to show a bright straight line running from the south-east edge of the nucleus north-westward, where it was met by a shorter bright line coming from the northern side of the nucleus. As Mr. Maw saw it but faintly illuminated on the 15th, and the Greenwich photograph of that day does not show it at all, we must conclude that it lasted only for a few hours with the brilliancy clearly shown in our photograph.

On the morning of the 16th about 8 A.M. I saw perfectly well what I called above "a half dark jetty running between two dark places." I saw equally well two bright filaments diving into the dark nucleus, one on the northern and the other on the south-eastern edge of the nucleus, just where the extremities of the late "bridge" had been resting.

Our photograph shows exactly the same, except that the "jetty" is so faint that it cannot be printed on paper. This observation was made several hours before Mr. Maw could have observed. Therefore it is not impossible that the said bridge was restored during the time. "He was," he said, "much surprised to find this curved bridge restored to its original brilliancy." Though I have no positive proof of this peculiarity, and never saw the bridge restored after the 15th, I should not be very much astonished if it had been. Indeed, on the morning of the 17th, the two bright markings, one on the north side, the other on the south-east side of the nucleus, though separated by a large space, were, however, diving more profoundly into the nucleus, as if they were to connect again. One thing is certain, the "bridge" sketched and photographed on the 15th was quite different from that of the 14th. It cannot therefore be supposed that the brilliant curved "bridge" of the 14th had remained unchanged until the 16th, only hidden from us by a dark cloud-like matter floating above.

In support of my assertions I beg to enclose a copy of my drawings and photographs. Yours very truly,

S. Chevalier.

Zo-sè Observatory, Zi-ka-wei,
1905, September 20.

Gentlemen,—

I am much obliged to you for forwarding to me a proof of the interesting letter which you have received from Father S. Chevalier, and I have also to thank Father Chevalier himself for kindly sending to me direct a set of the photographs which accompanied his letter to you. The set includes five photographs of the whole solar disc taken on July 13, at $7^h\ 48^m$ A.M.; on the same date, at $3^h\ 54^m$ P.M.; on July 14, at $7^h\ 50^m$ A.M.; on July 15, at $7^h\ 51^m$ A.M.; and on July 16, at $8^h\ 3^m$ A.M., respectively. The other prints are photographs of four large scale drawings of the big spot taken on the same dates and in each case at 8 A.M. The direct photographs show the solar disc to a scale of 2½ in. to the Sun's diameter, and the definition is most excellent. The large scale drawings have evidently been made with great care and show a very large amount of detail.

It will be seen that Father Chevalier's observations were made chiefly about 8 A.M., while mine, referred to in my last letter, were all made about 4 P.M. This difference in the time of day, combined with the difference of longitude of the two stations, means that there was a difference of about sixteen hours between Father Chevalier's observations and mine, a considerable period in the history of a sun-spot. Nevertheless, a comparison can be made. Father Chevalier's drawing of July 14 shows the "bridge" much as I saw it sixteen hours later, except that the loop form which was such a special feature of this bridge was then more developed. His drawing of the 16th also shows the "bridge" almost exactly as I saw it on the same date, except that he shows a portion of the loop partially obscured, while when I examined it sixteen hours later it was perfectly clear and bright throughout its entire length. So far our observations are in accord.

With respect to the conditions on July 15, however, the case is different. Father Chevalier's drawing on that day shows no trace of the delicate loop, but indicates, on nearly the same site, a "bridge" consisting of two nearly straight portions joining at an obtuse angle, and he suggests that it was the second "bridge" which developed into the "loop" seen on the 16th. I am, however, unable to accept this explanation, and the direct photograph taken on the 15th seems to me, when examined in a strong light with a lens, to support my view. It may be remembered that when I examined the spot on the 15th, sixteen hours after this photograph was taken, the loop was still visible, but was very faintly illuminated. I saw no trace of the second "bridge" referred to above.

I should like to take this opportunity of thanking Prof. Michie-Smith for the information respecting this spot which he gives in his letter published on p. 388 of your October issue. His notes regarding the spectroheliograms taken on July 14 to 17 are very interesting, particularly the fact that on the 17th the spectroheliogram showed two bridges instead of only one as in the ordinary photograph.

Yours sincerely,
W. H. Maw.

Kensington,
1905, Nov. 15.

An Indian Mode of Indicating Time.

Gentlemen,—

Looking through the 26th volume of the *Observatory* last night my attention was caught by a paragraph in the "Oxford Note-Book" on p. 470 (December 1903), on the method employed by Australian savages to mark the time of day at which they passed a particular spot. During my recent visit to Canada I had the great pleasure of meeting Dr. Hind, the explorer of Assiniboia and of part of the Labrador peninsula. (Dr. Hind is first-cousin to the late Superintendent of the 'Nautical Almanac.') Dr. Hind told me that he found in his great exploration of the Moisie River that his Indian guide would habitually put up a sun-mark for the benefit of the members of the expedition who were following at a few hours' distance, to show how far the first party was ahead. This would take different forms according to the local conditions, but the typical one consisted of a short stake fixed upright in the ground, with a stick lying at its foot and *fitted to its shadow at the time.* He had further found on enquiry that it was the usual practice amongst the Nascaupee and Montagnais Indians.

It is easy to see how this simple and efficient method of sun-marking might be supposed by a careless enquirer to be intended by the native as a charm to "stop the Sun" if its meaning were explained to him by a native who had little knowledge of English, or whose own language the enquirer did not himself understand. Not a few of the habits, customs, and beliefs ascribed to savage races have owed their entire origin simply to "bad language"—the difficulty of communicating ideas when neither side really understood the speech of the other.

Royal Observatory, Greenwich,
1905, Oct. 25.

Yours faithfully,
E. WALTER MAUNDER.

Names for Satellites.

[Translation.]

GENTLEMEN,—

Mr. Whitmell's letter in the *Observatory* of August last asks for a name for Neptune's satellite discovered sixty years ago. Neptune was not without sons, of whom the most renowned was Triton, the constant companion of his father on the ocean.

This name has already been adopted by several astronomers, notably by M. Flammarion in his 'Astronomie Populaire,' which appeared in 1880, a quarter of a century ago. The Editor of the Publications of the Society of the Pacific has also adopted this very logical denomination.

I am, Gentlemen,
Yours faithfully,
M. FOUCHE,
Secretary Astronomical Society of France.

Paris, 1905, Nov. 9.

Velocity of a Planet at any Point.

GENTLEMEN,—

Will you allow me a word in response to the note of Mr. Robbins in your October number? Mr. Robbins certainly establishes beyond question Dr. Whewell's priority in respect to the theorem hitherto generally ascribed to Van der Kolk; but the statement that, in so ascribing it, Prof. Young and Dr. Moulton "*ignore*" Dr. Whewell I feel to be objectionable and unjust. The word "ignore," as ordinarily used, implies deliberate intention to disregard something known. Speaking for myself, I simply was not aware of the existence of the corollary quoted by Mr. Robbins until I read his note—ignorance which I regret, but do not recognize as wicked.

Yours very truly,
C. A. YOUNG.

Hanover, New Hampshire,
1905, Oct. 21.

The Visibility of the Helium D_3 *Absorption-line in the Solar Spectrum.*

GENTLEMEN,—

Following the several reports in your columns on the appearance of the helium D_3 absorption-line in the solar spectrum, there is, in the September issue of the *Observatory*, a letter from Prof. C. Michie-Smith, of Kodaikanal Observatory, India, in which he announces his intention of publishing a list of observations of D_3 as a dark line in the solar spectrum. This list has now come to hand under the name of 'Kodaikanal Observatory Bulletin No. III.,' and constitutes a full confirmation of the recent observations of D_3 made by Prof. A. Fowler and the writer of this letter. The list in question enumerates some 80 dates of observation of D_3 since March 1904 up to May 1905, and since that date I have made at least 50 more observations, and no doubt Prof. Fowler as well, as recently he wrote to me "it has now practically ceased to be anything very remarkable."

The three different and independent series of observations of D_3 are in practical agreement as regards the following features of the phenomenon:—

(1) That a really agitated spot-group is next to certain to show D_3, often both bright and dark, synchronous with the geyser-like repeated paroxysms.

(2) That D_3 will mostly be seen outside and in the rear of a spot, or in the intervening area of a spot-pair, and sometimes over quite tremendous areas and entire groups.

(3) That the hydrogen lines as a rule give the cue to the likely presence of D_3 by their extremely distorted appearance, but exceptions were also noted by all three observers, and the hydrogen lines were seen quiescent, while D_3 was conspicuous, and *vice versa.*

(4) The appearance of the helium D_3 absorption-line is generally one of irregularity and knottiness, sometimes weak and ill-defined, at other times strong and well-defined. By far in most cases we have seen it dark to very dark.

(5) The cases where D_3 can be traced "*through*" a spot are comparatively rare, and personally I think that, except seen in a spot on or near the central meridian, I should be inclined to attribute this *through* appearance rather to perspective. Anyhow, by far the majority of cases show D_3 as stated under the second of the above-mentioned general features.

(6) That sometimes D_3 is seen partaking of the distortion of the hydrogen lines to the extent of being broken up and torn up into detached fragments, of which an exceptionally fine instance occurred on July 14 last at 6.50 A.M. local civil time. Prof. Michie-Smith also records several such instances in his list.

During the passage of one of the recent spot-groups, which was just past being central last Wednesday, the 15th inst., D_3 was a conspicuous feature from the day of the appearance of that group on the eastern edge, and afforded on the 7th inst. quite an exceptional sight at intervals. Prof. Fowler's experience is identical with my own, and he says:—"I have seen D_3 *nearly every day* on which I have examined *all* the spots, since we [he and I] were last in communication."

It will be interesting to follow this matter up with a view to see whether this now so frequent appearance of D_3 is a feature of a particular stage of the activity-cycle now in progress and near its maximum, or whether it is a characteristic of certain limited latitudes or zones. The fact remains, however, that D_3 is much more frequently visible than was at one time believed. Prof. H. Kayser, of Bonn University, wrote to me yesterday how surprised he was to hear that D_3 is seen so frequently, and in view of the new facts we shall have to readjust our views on the behaviour of helium in the Sun's spectrum.

Assuming the induction effect causing the terrestrial magnetic storm to emanate from the most excited areas on the Sun, what is stated under (2) may explain the often observed fact that the terrestrial magnetic disturbance sets in *after* the leader-spot has crossed the central meridian.

Yours respectfully,

Albert Alfred Buss.

2 Lansdowne Terr., Grosvenor Sq.,
Ashton-on-Mersey, nr. Manchester.
1905, Nov. 22.

NOTES.

Comet Notes.—A comet of the seventh magnitude (1905 *b*) was discovered by M. Schaer at Geneva on Nov. 17. It was only 4° from the North Pole, and was moving at the very rapid rate of 9° daily; the elements show that it was then only about 22 millions of miles from the Earth, and moving in the opposite direction.

Elements by Herr Ebell.

T	1905 Oct. 27·49
ω	135° 39′
☊	223 45
i	138 55
log *q*	0·0263

Ephemeris for Berlin Midnight.

	R.A.	Dec.		R.A.	S. Dec.
	h m s	° ′		h m s	° ′
Dec. 2 ..	23 30 49	1 12 N.	Dec. 10....	23 31 28	8 36
6 ..	23 30 49	4 47 S.	14....	23 32 37	10 12

The brightness declines rapidly, and on Dec. 14 is about $\frac{1}{17}$ of that at discovery.

Herr Svedstrup, who died in 1893, left among his papers a nearly complete investigation of the orbit of Comet 1886 I., which was interesting from its hyperbolic motion and its near approach to the Earth. This has now been published by Herr Thiele, and seems to be practically complete; the author, however, contemplated the investigation of the systematic errors of the different observers, and a re-computation of the perturbations, but probably the elements would not be greatly altered.

T	1886 April 5·9915 Berlin M.T.	
π	162° 56′ 51″	1886·0.
ω	126 34 34	
☊	36 22 18	
i	82 36 59	
q	0·642378	
e	1·000457	

A. C. D. C.

Minor Planet Notes.—There have been a very large number of discoveries at Heidelberg:—

Planet.	Date.	Discoverer.	Planet.	Date.	Discoverer.
RC ...	1905 Sept. 19	Wolf.	RR ...	1905 Nov. 3	Wolf.
RD ...	19	,,	RS ...	Oct. 26	,,
RE ...	19	,,	RT ...	26	,,
RG ...	1903 Sept. 22	Dugan.	RU ...	26	,,
RH ...	1905 Oct. 20	Wolf.	RV ...	26	,,
RJ ...	20	Kopff.	RW ...	26	,,
RK ...	23	Wolf.	RX ...	Nov. 1	,,
RL ...	24	,,	RY ...	1	,,
RM ...	25	,,	RZ ...	1	,,
RN ...	24	,,	SA ...	3	,,
RO ...	25	,,	SB ...	3	,,
RP ...	25	,,	SC ...	3	,,
RQ ...	26	Kopff.	SD ...	3	Kopff.

RA is identical with 429; the existence of RJ is doubtful; RK is probably 509 Jolanda; RN possibly 477 Italia.

The following planets have been named:—

Number.	Letter.	Name.
406	CB=QU	Erna.
520	MV	Francisca.
522	KN=NC	Helga.
542	OQ	Susanna.
544	OU	Jetta.
565	QN	Marbachia.
566	JF=QO	Stereoscopia.
568	QS	Cheruskia.

The last named is identical with an unnamed planet discovered by Wolf 1891 Dec. 1. Helga has been (probably) detected on a plate taken 1901 Aug. 23.

554 Peraga has been detected on a photograph of the Pleiades taken 1900 Sept. 30. The planet was stationary and left no trail, hence it was not detected till the orbit was known. A. C. D. C.

OBITUARY.—RALPH COPELAND. Dr. Copeland, late Professor of Astronomy in Edinburgh University, and Astronomer Royal for Scotland, whose death was briefly announced last month, had had a varied career. He was born on September 3, 1837, near Woodplumpton, Lancashire, and was educated at the Grammar School of Kirkham. He emigrated to Australia, and spent several years as a shepherd, but afterwards was drawn to the gold-diggings, during which time his attention was turned to the study of astronomy. He returned home in 1858, and on the voyage observed Donati's Great Comet of that year. On reaching England, Mr. Copeland became an apprentice to a firm of locomotive engineers, and with some others employed in the firm he founded a small observatory. In 1864, trade in Lancashire being bad, Copeland decided to go to Germany to study astronomy, and matriculated at the University of Göttingen. Here he offered himself as volunteer assistant at the Observatory, and, in conjunction with Carl Börgen, made the Göttingen Catalogue published in 1869. The next experience in this varied career was a journey to Arctic regions, for the subject of this note went as a member of a German expedition to explore the coast of East Greenland to make meteorological and magnetic observations, and for this service he received the order of the Red Eagle from Emperor William I.

After his return from this expedition Dr. Copeland took up the position of Assistant to the present Earl of Rosse at Birr Castle, where he spent three and a half years, one important item of his work being the measurement of the radiant heat of the Moon with the 3-foot reflector. At the end of that time, that is in 1879, he took up the appointment of Assistant at the Dunsink Observatory, of which Sir Robert Ball was then Director. In the same year he was a member of Lord Lindsay's unsuccessful expedition to Mauritius to observe the Transit of Venus, and eight years later he went on a similar object to Jamaica.

In 1876 he accepted the offer of Lord Lindsay (now the Earl of Crawford) to take charge of his Observatory at Dunecht, and during this time his name became familiar to astronomers by reason of the 'Dunecht Circulars,' signed Ralph Copeland, which were distributed from that observatory,. giving information of cometary and other discoveries and orbits computed by himself. In 1889, on the retirement of Prof. Piazzi Smyth, Dr. Copeland succeeded him as Professor of Astronomy at Edinburgh and Astronomer Royal for Scotland. The removal of the Observatory from Calton Hill to Blackford Hill, and the installing of the equipment, then much enlarged by the addition of the instruments from Dunecht, were effected under his direction. Since that date Dr. Copeland has been on several expeditions to observe eclipses, using a telescope of 40-feet focal length; but this year his health, which had been failing him for some time, did not permit him to attempt to observe the solar eclipse. Dr. Copeland contributed several papers to the proceedings of the Astronomical Society (of which he was elected a Fellow in 1874), mostly on spectroscopic subjects. He died on October 27th last, in his 69th year.

A FINE AURORA, SUN-SPOTS, AND MAGNETISM.—On the evening of November 15 a splendid example of the Aurora Borealis was observed from many places in Great Britain. At Aberdeen * it was noticed about 5.30 P.M., and at that time it consisted of an arch of greenish-white light low down on the north horizon, the centre of the arch bearing approximately N.N.W. This arch rose gradually, and as it did so streamers of a brilliant greenish white began to shoot upwards towards the zenith. The streamers increased in number and finally became a more or less continuous curtain swaying gently. When the arch had reached an altitude of about 45° it suddenly became of a bright rose colour, and then deep crimson, the curtain of streamers still remaining greenish white. A narrow band of pulsating white light now formed in the E.N.E. and increased into an arc lying E.N.E. and W.S.W., parallel to, and a little above, the crimson arch. Shortly afterwards the aurora faded rapidly and had disappeared altogether by 6.30 P.M.

At Greenwich the Aurora was first seen † at $7^h\ 55^m$ as an elongated patch in the N.W. at an altitude of from 20° to 30°. At $8^h\ 50^m$ the Aurora assumed the appearance of a complete arch stretching from N. 40° E. to about N. 85° W., and was of a brilliant red hue. At 9 o'clock the arch had split up into two distinct portions, one a very diffused mass in the N.E., and the other in the N.N.E., which shortly afterwards assumed a vertically banded appearance. Various streamers and diffused glows were seen till about $9^h\ 35^m$, at which time a very faint patch was seen extending from the zenith towards Polaris. Nothing further was seen after a quarter to 10.

This Aurora, it may be noticed, took place about two days before the return to the central meridian of the Sun of the region in which the great sun-spot of October 14–27 had formed. The great spot did not return this month, at least not in a recognizable shape, but two considerable streams of spots had formed preceding its place. One of these was a little ahead of the central meridian, the other not quite up to it at the time of the Aurora.

On November 15 the magnets at Greenwich showed a disturbance greater than has previously been experienced this year. A magnetic storm began about 3 in the afternoon, causing sharp waves in the traces, especially about half-past six and nine o'clock

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* This account is extracted from a letter written by Mr. G. H. Clarke, of the University Observatory, Aberdeen.

† This description is by Mr. G. S. Barrett, of the Royal Observatory.

in the evening, when synchronous waves occurred in the records of all three elements—Horizontal Force, Declination, and Vertical Force—the declination-needle showing a deflection of about 40′ at the later time.

ORIGIN OF PLANETARY SYSTEMS.—An alternative to Laplace's theory of the formation of planetary systems is suggested by Mr. F. R. Moulton, of Chicago University, in the *Astrophysical Journal.* The theory is that the planets and their satellites have been formed around primitive nuclei of considerable dimensions existing in a spiral nebula probably similar to those which Prof. Keeler showed to be many times more numerous than all the nebulæ of other types. The growth of each nucleus was caused by the gradual accretion of smaller masses, and the method of this growth which is suggested accounts for all the different types of bodies now found in the Solar System, and for their present motions and velocities on dynamical principles. The original spiral nebula is supposed to have been formed by the near approach of another star to the body which is now our Sun. This exterior attraction set up tides in the solar matter, and, being continued, actually caused immense masses to be ejected and drawn out into the spiral form. On this assumption the spiral would emerge from the central nucleus in two directions, on opposite sides, and this is the form generally shown on photographs of such nebulæ.

NOVEMBER METEORS.—So far as we have yet learned there was no display of either Leonid or Bielid Meteors during November. Watch was kept at Greenwich for Leonids on the nights when these showers were expected, most of them being overcast, but on November 18, when the Bielids might have been seen, there were practically no meteors.

SYMONS GOLD MEDAL.—The Council of the Royal Meteorological Society have awarded the Symons Gold Medal to Lt.-Gen. Sir Richard Strachey, R.E., G.C.S.I., F.R.S., in recognition of the valuable work which he has done in connection with Meteorological Science. The medal will be presented at the Annual General Meeting of the Society on January 17th, 1906.

ROYAL INSTITUTION.—A Christmas Course of Lectures, adapted to a Juvenile Auditory, will be delivered at the Royal Institution by Prof. Herbert Hall Turner, D.Sc., F.R.S., on "Astronomy." The dates of the Lectures are December 28, 30, 1905, January 2, 4, 6, and 9, 1906, at Three o'clock.

THE position of Regius Professor of Astronomy in the University of Edinburgh and Astronomer Royal for Scotland has been filled by the appointment of Mr. Frank Watson Dyson, M.A., F.R.S., who has been Chief Assistant at the Royal Observatory, Greenwich, since 1894. We, and we certainly speak for all the Greenwich staff, offer our sincere congratulations. Edinburgh's gain is a loss to Greenwich.

WE are glad to hear from M. Valle, of the Tacubaya Observatory, who has taken up a section of the Astrographic work originally assigned to one of the South American Observatories. M. Valle tells us that he has 1000 Catalogue plates taken and a zone of declination one degree broad nearly reduced, so that his work is going apace.

IT will be seen from an announcement in our advertisement pages that Mr. Franks, who helped the late Dr. Roberts for many years in taking his photographs, is at present without astronomical work. It will be a pity if the energies of such an able man are not speedily utilized.

Mr. H. F. NEWALL with Sir William Huggins, who retires from the Presidency, will represent Astronomy on the Council of the Royal Society in the coming year. Lord Rayleigh is the new President.

A FIRM of publishers is issuing as a sort of Christmas card a thing called a "Star Calendar," and said to be compiled by [H. P. H.]. May we take the opportunity to say that this does not refer in any way to one of the Editors of this Magazine.

THE list of honours conferred on the King's birthday, Nov. 9, includes the name of Prof. George Howard Darwin, F.R.S., who has been created Knight Commander of the Bath. We have much pleasure in offering our sincere congratulations.

THE 'Astronomischen Jahresbericht,' prepared hitherto by the late Prof. Wislicenus, will now be undertaken by Prof. Dr. Berberich.

A Cape telegram in the daily newspapers states that Sir David Gill has made a public announcement that he intends to retire from the position of H.M. Astronomer next year.

The "copy" of the "Oxford Note-Book" posted by Prof. Turner at Cairo in August last, which should have appeared in our September number, has not come to hand, although we believe the Post Office made efforts to trace it. As these notes described the early part of his eclipse expedition, Prof. Turner has kindly re-written them from memory, and they are issued with this number in the form of a Supplement, and paged so that they may be bound in this year's volume in their proper place after the September number.

In bringing this volume, which contains more pages than any previous one, to a close we would like to call attention to the many demands there have been on our space this year, and to point out that, for financial reasons, our space is strictly limited. We trust this will be taken as a sufficient reason for many conscious omissions of interesting matter—book reviews (we have the new edition of Miss Clerke's 'System of the Stars,' Schiaparelli's 'Astronomy in the Old Testament,' and other books on our table), notices of astronomical publications, and Observatory reports. It may be that we may find opportunity to remedy this and to give adequate accounts of these later; meanwhile we can only regard this excess of matter as a sign of the present strength of the science of astronomy.

Though our circulation is certainly not "the largest in the world," we think we may claim a circulation as "wide" as that of any other periodical. There has just been brought to our notice a copy of the Report of the Wanganui Astronomical Society—Wanganui is in latitude 39° 57′ S., longitude 175°·6 E.,—and at the Annual Meeting, held on October 18 last, reference was made to the *Observatory* for August, just arrived, with Mr. Wesley's drawing from the Lowell photographs of Mars.

The next Meeting of the Royal Astronomical Society will take place on Friday, December 8; of the British Astronomical Association on Wednesday, December 27.

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Supplement: From an Oxford Note-Book, Sept. 1905.

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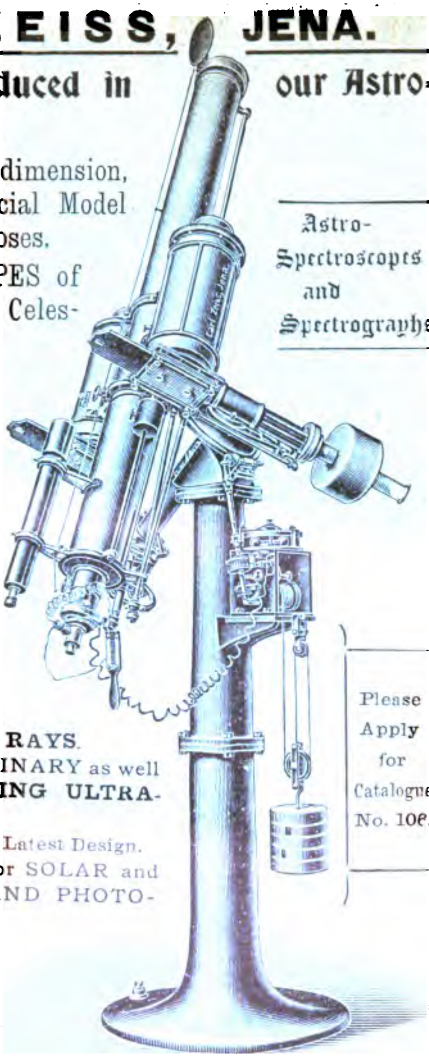

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